THE WORM RUNNER'S DIGEST

"Man is certainly stark mad; he cannot make a worm, and yet he will be making gods by dozens"

Michel de Montaigne
Essays, Book II

"Worm or beetle---drought or tempest
---on a farmer's land may fall,
Each is loaded full o' ruin, but a
mortgage beats 'em all."

Will Carleton
"The Tramp's Story"

"You have deliberately tasted two worms and you can leave Oxford by the town drain".

Cannon William Spooner,
dismissing a student from Oxford.

"'Is it weakness of intellect, birdie?' I cried,
'Or a rather tough worm in your little inside?'
With a shake of his poor little head he replied,
'Oh, Willow, titwillow, titwillow.' "

William Schwenck Gilbert
The Mikado

"Edible: good to eat, and wholesome to digest, as a worm to a toad, a toad to a snake, a snake to a pig, a pig to a man, and a man to a worm."

Ambrose Bierce
The Devil's Dictionary

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The Worm Runner's Digest

An Informal Journal of Comparative Psychology

Published Irregularly By
The Planarian Research Group
Department of Psychology
The University of Michigan
Ann Arbor, Michigan

March, 1961 THE WORM RUNNER'S DIGEST Vol. III, No. 1
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WORMS AND THINGS

*This is by far the largest issue of the DIGEST ever printed, representing, perhaps, the increase in activity of worm-runners around the world. Much has happened to us, and to the DIGEST, since our November issue. To begin with, BOB SOMMER's delightful "Einstein's Girl Friday" caught the attention of editors around the world and landed him (and the DIGEST) in such unlikely places as NEWSWEEK and THE AMERICAN BEHAVIORAL SCIENTIST. Some eight or nine other journals wrote asking for reprint rights, which we gave gladly. Then, due to the sterling promotion efforts of our friend BOB BEYERS in the University of Michigan News Service, the worms gathered a front page article in the New York HERALD TRIBUNE, written by EARL UBELL. We had long feared we'd end up on the front page, but never dared to hope under such favorable circumstances. This H-T article appeared also in the Paris edition (and embroiled us in a flurry of "Letters to the Editor") and got picked up by the London TELEGRAPH. The chain reaction continued as JANE CLAPPERTON read the TELEGRAPH article and wrote a saucy piece called "Politic Worms," which appeared in the February 1st issue of PUNCH. MIKE PARKAS, of Michigan's SPONSORED RESEARCH ADMINISTRATION, read the article and called it to our attention. We wrote PUNCH and obtained permission to reprint same, and you'll find it somewhere in the following pages. To all of our friends, all around the world, who have helped Promote the General Welfare and Interest of Worms, we give our sincere thanks.

*When to print, when to reprint, when to digest? In this issue you'll find a speech by ED WALKER which he gave in a hospital setting many years ago. We've always been quite fond of that particular talk (and of ED), and have handed out copies of the talk (we had them mimeographed on our own) to various and sundry students at Michigan. Finally we prevailed upon ED to let us print up the talk in this issue of the DIGEST. He agreed, providing we would point out that the speech was given some time ago and hence doesn't bring in all of the current evidence published since that time. The ideas presented in the talk are relatively timeless, however, and worthy of anybody's reading. In like fashion, we prevailed on M. E. BITTERMAN to let us reprint his talk on Comparative Psychology, printed in a recent issue of the AMERICAN PSYCHOLOGIST. Since not all of our readers (by half) read the A-P, we felt it our duty to bring them this excellent paper. Along similar lines, we talked our Michigan colleague JOHN BARDACH into letting us publish a talk on fish behavior he presented recently at a meeting in Toronto, which fits well with BITTERMAN's paper.

We are still faced with the problem of what to do about research reports which have been accepted for publication in the more staid journals which we'd like to bring along to our own readers. Sometimes we take it upon ourselves to condense these papers, sometimes (as is the case with JAY BOYD BEST's report in this issue, which in more complete form has been accepted for publication in the JOURNAL OF COMPARATIVE AND PHYSIOLOGICAL PSYCHOLOGY) we twist the authors' arms and get them to do the re-write chores. In the case of ED HALAS' paper, also soon to appear in JCPP, we decided to let HALAS speak for himself and bring you herewith the Summary and Conclusions of his JCPP paper:

SUMMARY

The present study was undertaken to determine the type and number of responses elicited in planaria by light. Four groups of animals were given 150 training trials under four different conditions: (1) light-shock pairing, (2) light-no shock, (3) no light-shock, and (4) no light-no shock. After 16-22 hours of rest, these groups were given 25 test trials to light to determine the number and types of responses elicited in planaria by light. A fifth group was used as a control to determine the number and types of responses emitted by planaria under normal conditions. It was found that light was capable of eliciting two types of responses: (1) cephalic turning and (2) longitudinal body contraction. The turning response was preferred over the body contraction. The findings were discussed in relation to past studies using light as a neutral or conditioned stimulus in classical conditioning experiments.

For further information, write directly to Dr. HALAS, Department of Psychology, University of North Dakota, Grand Forks, North Dakota.

*The worms have been blessed with visitors aplenty of late, a condition that warms our hearts immensely. ARTHUR KOESTLER, better known for "Darkness at Noon" than for his long-standing interest in flatworms (he became interested in planaria after reading CHILD's classic publications in this area), spent several days in Ann Arbor in February and now promises to Tell England All in a forthcoming issue of THE OBSERVER. KOESTLER we found to be an exceedingly charming man, the very model of a modern intellectual, with an astute grasp of comparative and physiological psychology. HARRY HELSON, long-time mentor of the Editor's, flew up to talk at Michigan and we trapped him into several long chats about our work. GREGORY KIMBLE (Duke), CHARLES ERIKSEN (Illinois) and BETTY HORENSTINE (NIMH) flew in to inspect our laboratory the end of February, and we spent several most pleasant hours with these three Ph.D.'s discussing the present state and future of worm running. Thanks in no small part to their good offices, we suspect, NIMH has seen fit to extend our current research grant for another two years. RICHARD TEEVAN, who was finishing up his degree at Michigan about the time the worms got started here, drove in from Bucknell the middle of March to consult on the Freudian motivations lying behind worm running.

When people weren't visiting us, we were visiting people. Hence, the Editor took a trip to Brookhaven National Laboratory the end of January at the invitation of LEROY AUGENSTINE, to proselytize a little on his favorite experimental animal. Chats with HENRY QUASTLER, in the Biology Department at Brookhaven, and later with SIMON FREED, in the Department of Chemistry, led to the projected undertaking of some joint biochemically-oriented research during the coming summer. If all goes well, WILLIAM CORNING, of Rochester, will spend the summer at Brookhaven, enjoying the breezes from Long Island Sound and conditioning planaria, while Drs. QUASTLER and FREED undertake (among other things) to trace the migration of regenerator cells throughout the worms' bodies by use of radioactive isotopes. We will jet in to New York to follow their progress as often as United's 720's will take us there. This prospect of working with a covey of bright scientists in disciplines other than Psychology pleases us immensely.

We are also scheduled to propagandize a little at the University of Indiana the end of March and look forward to winning new converts to wormism and to seeing many old friends again.

*ALLAN L. JACOBSON, of the Planaria Research Group, Department of Psychology, University of Michigan, has just completed an extensive review of **all** the extant literature on Learning and Behavioral Modification in flatworms and annelids (earthworms). We hope to have it typed up on dittos by the middle of April. Anyone who wishes a pre-publication copy of this review article may have same by writing to Mr. JACOBSON.

*Moving? If so, please let the DIGEST know. We have more trouble keeping track of our "subscribers" than we do of our worms.

*JAY BOYD BEST writes that he is, among other things, busily engaged in constructing a Skinner box for planaria. According to him, "This last device is known as 'Best's Folly' in and around the instrument shop. If these little beasts could be taught to work a manipulandum it might be possible to achieve a stable type 2 conditioned response pattern and also to work, via automation, within a time scale that is more appropriate to the planarian than the limit of patience of the experimenter. In addition, I feel the automatic scoring and programming of the Skinnerian situation would answer those critics who dislike the subjective element in the scoring procedures." We wish JAY the best of luck, and will order a dozen of "Best's Follies" once they're on the market. But we keep wondering, late at night as we try to go to sleep, just what will the worms push that lever with.

*For our new readers, the translation of our motto "Ignotum per Ignotius" reads "The Unknown Explained Through the Still More Unknown," and still is apropos of our approach to things. The cover of this issue is, as usual, by our favorite Cover Girl, REEVA JACOBSON. The typing, spelling, etc., by our favorite amanuensis, ANGELA THOMPSON. Blessings on them both.

*Good reading!

James V. McConnell
Editor

TITLE: Towards a Comparative Psychology of Learning*

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In the early years of this century, when the experimental study of animal intelligence was just getting under way, many different species were brought into the laboratory. From the very beginning, to be sure, there was some preference for the higher animals, but, on the whole, early interest ranged rather widely up and down the phylogenetic scale. Before long, however, the scope of research narrowed. Attention became fixed on a small number of mammalian forms, which were chosen primarily for reasons of convenience, and treated as representative of animals in general, with the cheap and docile rat easily leading all the rest.

A set of curves which nicely illustrates this trend was published some years ago by Beach (1950). Based on a count of papers appearing between 1911 and 1948 in the Journal of Comparative and Physiological Psychology and its forerunners, the Journal of Comparative Psychology and the Journal of Animal Behavior, the curves show how interest in the rat mounted rapidly, while interest in the submammalian forms declined. In the thirties, a stable pattern emerged, about 60% of the papers dealing with the rat, about 30% with mammals other than the rat, and about 10% with the lower forms (the submammalian vertebrates and the invertebrates). If we make the computations required to bring these curves up to date, we find no significant change in the state of affairs decried by Beach a decade ago. Please note that Beach's curves are based on all papers published in a single journal. If we count only the papers on learning, and look at a broader sample of journals, the effect becomes even more striking. About 90% of our work on animal learning has been done with the rat.

Of course, specialization has its advantages, and, if the process of learning were essentially the same in all animals, it would be rather improvident of us to diffuse our efforts over the phylogenetic scale; but the only way to find out whether the process of learning is essentially the same in all animals is to make some careful studies at widely separated points in the scale. We have not made such studies. It is our willingness to assume that the process of learning is essentially the same in all animals which has been responsible in large measure for our concentration on the rat.

The assumption comes to us from Darwin, who was interested in psychological as well as in structural continuity. For Darwin, novelty was incompatible with continuity, and his purpose was to demonstrate that any seemingly unique property which made its appearance in the animal series really was not unique at all--that some hint or promise of it always could be discovered at an earlier

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*This paper is adapted from an address to the Eastern Psychological Association at its 1959 meeting. The research described has been supported by Grant M-2857 from the National Institute of Mental Health and Contract Nonr-2829(00) with the Office of Naval Research. Reprinted from the American Psychologist, 1960, 15, 704-12, by permission.

point in the series. On the basis of the only evidence available to him--the tall tales of naturalists and zookeepers--Darwin decided that phylogenetic differences in intelligence were differences "of degree and not of kind". Not even in the intelligence of man could Darwin find anything new. Capacities which seemed peculiar to man, he explained, could be traced to man's use of language, which itself--"half-art and half-instinct"--clearly bore "the stamp of its gradual evolution" (1871, pp. 105-106). Darwin's formula for bridging the gap between man and the infrahuman animals has a contemporary flavor; it finds wide use even today.

The early comparative psychologists were not entirely convinced by Darwin's arguments. Critical of his anecdotal evidence, and proud of their own new methods, they were determined to have a look for themselves; but their skepticism did not last long. Observing a variety of animals in problem boxes, mazes, and discriminative situations of one sort or another, they were more impressed by the similarities in behavior than by the differences, and quickly succumbed to the Darwinian view. Thorndike, himself, showed the way. At the outset, his contempt for certain features of Darwin's position was undisguised--"man was no more an animal with language," he wrote, "than an elephant was a cow with a proboscis" (1898, p. 87)--but, prepared though he was to find important phylogenetic differences in mode of learning, he found none. His very earliest observations led him to the "working hypothesis" that the process of learning was essentially the same throughout the phylogenetic series. All animals, for Thorndike, were "systems of connections subject to change by the law of exercise and effect" (1911, p. 280), differing only in the particular connections formed and in the ease of connection. The influential Watson took the same position, although he rejected the law of effect and tried to get along with exercise alone--"in passing from the unicellular organisms to man," said Watson, "no new principle is needed" (1914, p. 318)--and gradually Thorndike's working hypothesis became transformed into an article of faith. The transformation is reflected in Beach's curves. As a working hypothesis, the proposition that learning is essentially the same in all animals led to the study of many animals. As an article of faith, it led, by the principle of least effort, to concentration on one.

Specific illustrations of the contemporary faith are not hard to find. Dollard and Miller base their account of human personality and psychotherapy on the assumption that, as they put it, "any general phenomena of learning found in rats will also be found in people" (1950, p. 63). They admit that "people may display additional phenomena not found in rats," but at no point in their book can one find any indication that they take the possibility very seriously. What they refer to as "the higher mental processes" of man they explain in terms of mediating responses, primarily linguistic in character, to which the general laws of stimulus-response connection are assumed to apply.

Another interesting example of the contemporary faith was provided by Skinner a few years ago, when, in an address to the Eastern Psychological Association, he attempted to justify his disregard of the standard statistical procedures. Those procedures are necessitated, he said, only by the fact of individual differences, and they lose all point when individual differences are eliminated "in advance of measurement". To demonstrate the extent to which his own method eliminates irrelevant sources of variability, he presented, among others, the curves shown in Figure 1. (See end of article for all Figures.) The curves were made by three animals trained in three different laboratories on a multiple fixed-interval-fixed-ratio schedule of reinforcement--one by a

pigeon, one by a rat, and one by a monkey. "Pigeon, rat, monkey, which is which?" Skinner asked, and he answered:

> It doesn't matter. Of course, these three species have behavioral repertoires which are as different as their anatomies. But once you have allowed for differences in the ways in which they make contact with the environment, and in the ways in which they act upon the environment, what remains of their behavior shows astonishingly similar properties. Mice, cats, dogs, and human children could have added other curves to this figure. And when organisms which differ as widely as this nevertheless show similar properties of behavior, differences between the same species may be viewed more hopefully. Difficult problems of idiosyncrasy or individuality will always arise as products of biological and cultural processes, but is it the very business of the experimental analysis of behavior to devise techniques which reduce their effects except when they are explicitly under investigation (1956, pp. 230--231).

The function of these curves, the context suggests, was not to demonstrate that the process of learning is essentially the same in pigeon, rat, and monkey; that was assumed. The function of the curves was to demonstrate the power of the method, which was found in the fact that it reveals the essential similarity of the three animals despite substantial sensory and motor "idiosyncrasies".

Whatever Skinner's purpose, his curves do, of course, show some interesting similarities in the learned behavior of quite different animals, and evidence of the same sort has been accumulating since the turn of the century. A comparable set of curves dating back to the years between 1901 and 1904 is shown in Figure 2. Like those of Skinner, they represent the work of three independent investigators working with three different animals--a monkey, a rat, and a bird (in this case, a sparrow). The apparatus employed was the Hampton Court maze, as popular in its time as Skinner's box is today. "Monkey, rat, sparrow, which is which?" it was asked at the turn of the century, although the question went to the nature of phylogenetic differences rather than to the power of the maze method, and the answer came: "It doesn't matter." That answer is no more warranted now than it was 50 or 60 years ago. Attractive as the Darwinian hypothesis may seem, it should not be accepted on the basis of superficial resemblances of this sort. The resemblances may, in fact, be more than superficial, but we shall never know until our level of inquiry becomes more than superficial.

Our skepticism must, of course, be based to a considerable extent on some guesswork about the significance of phylogenetic differences in brain development. Thorndike, too, was interested in brain development, but he was inclined to attribute to it a purely quantitative significance; he early speculated that brain development meant merely the capacity for a greater number of connections and greater efficiency of connection. Even in recent years this kind of thinking has been common--for example, Miller (1951) has suggested that brain development may have no more significance than the addition of banks of keys to an automatic calculator--but brain development is more than an increase in the mass of brain tissue. New structures, such as the cortex, have appeared, which suggest the possibility of new functions. It is interesting to note, by the way, that the acceptance of the Darwinian hypothesis was accompanied by a growing disregard for the facts of brain structure. After giving up their inquiry into animal consciousness, the early comparative psychologists were at some pains

to establish that there still remained to them an area of investigation different from that of the zoologists, and that their methods and findings were as important as those of the zoologists. If some primitive experiments with a maze or a problem box suggested that the process of learning was the same in monkey and sparrow, why then perhaps the difference in brain development had no fundamental significance. That was the attitude which Watson expressed when, speaking of man's frontal lobes, he said that "simply because we have the tissue" it does not necessarily "have a life and death significance" (1914, p. 320). To be sure, we must agree with Watson in principle. If extensive and detailed investigation at a number of widely separated points in the phylogenetic scale reveals no fundamental difference in the mode of learning, we must sooner or later conclude that differences in brain structure have no relevance for mode of learning; but we should not be quite as easily persuaded as Watson.

Of course, not everyone has been as easily persuaded. From time to time, skeptical voices have decried the facile acceptance of Darwin's hypothesis and have urged the development of a systematic comparative psychology, but they have not often been able to propose a reasonable way of going about it. One of the most respected advocates of comparative research was Yerkes (1917), whose approach was that of the mental tester. Yerkes' idea was to develop a standardized situation appropriate to the sensory-motor capacities of a variety of species, and to order the species on the basis of their performance in it. This seems to be the common conception of comparative psychology even today. For example, following the lead of Harlow (1949), a number of investigators recently have been comparing raccoons, children, chimpanzees, and monkeys of varying description in terms of their rates of improvement in long series of discriminative problems. No wonder there has been little enthusiasm for comparative psychology so conceived. When I mention to someone that I am making comparative studies of learning, his first question is apt to be, not "How are you going about it?" but "How do you know that your measures are comparable?" He assumes automatically that I am comparing the animals in terms of certain absolute measures of performance from which I propose to infer differences in ability, and he wonders, then, how I can ever be sure that differences in performance are due to differences in ability rather than to sensory, or to motor, or to motivational differences. I sometimes find difficulty in convincing such a person that there is another kind of comparative research.

Suppose that I introduce a target into the living tank of a fish and reward the animal with a pellet of food for pushing against it.* The measure of performance is latency of response--the number of seconds which elapse between the introduction of the target and the animal's response to it--and I plot a curve showing change in latency over a series of reinforced trials. After the latency of response has fallen to a stable level, I terminate reinforcement and plot the progressive increase in latency which ensues. This procedure is directly analogous to one which has been used rather widely with the rat, and the results obtained--the acquisition and extinction curves--are quite similar to those for the rat. The absolute latencies may be very different in the two animals, but the relation between latency and trials is very much the same under conditions of consistent reinforcement and nonreinforcement, and it is the relation in which I am interested. In the past 50 or 60 years, we have begun to

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*For methodological details, see Longo and Bitterman (1959).

work out many such relations for the rat, and our theory of learning in the rat represents an effort to find order and meaning in those relations. I am interested now in the extent to which similar relations are to be found for the fish, because I am interested ultimately in determining whether a single theory will fit both animals.

Here, then, is the plan: Taking the much-studied rat as a point of departure, I select for comparison another animal--a fish--which is different enough from the rat to provide a marked phylogenetic contrast, yet similar enough to be studied under analogous conditions. The two animals are not to be compared in terms of their absolute scores in some standard apparatus. Work with the fish, like work with the rat, is to be directed at the discovery of functional relations. Its goal is a theory of the fish with which to compare the theory of the rat (Bitterman, Wodinsky, & Candland, 1958).

This conception of the comparative psychology of learning (as an attempt to determine whether learning in different animals may be understood in terms of the same set of laws or whether different laws are required) is not a new one. It was implied in the writings of Thorndike, and in the work of a small number of subsequent investigators, such as Schneirla (1946), who continued to take the comparative problem seriously. It was stated quite clearly by Hull (1945), although he himself did not give much weight to the possibility that different laws might operate at the different phylogenetic levels. As yet, however, there has been no explicit attempt to deal with a methodological difficulty which has seemed to stand between this conception and fruitful research--a difficulty similar to that by which the mental-test strategy is utterly defeated.

To the extent that functional relations of the same kind appear in fish and rat no one is troubled. The principle of parsimony leads us all to assume--though perhaps incorrectly--that the same process of learning is operating in the two forms. Suppose, however, that different functional relations appear. We may not be as ready then to infer that the underlying processes of learning are different. We may wonder, and with good reason, whether some peripheral factor--sensory, or motor, or motivational--is responsible for the discrepancy, and we may despair of ever being able to control such variables in comparative experiments. How should we hope to find a situation whose sensory and motor demands upon our fish are equivalent to those which commonly prevail in experiments with the rat? Fortunately, the problem is not an insoluble one. While the prospects for <u>control by equation</u> are slim indeed, there is available a perfectly suitable alternative, which we may speak of as <u>control by systematic variation</u>.

Consider, for example, the paradoxical relation between consistency of reinforcement and resistance to extinction which has been established in a variety of experiments with the rat, and suppose that a like effect fails to appear in an analogous experiment with fish--that a partially reinforced group of fish extinguishes <u>more</u> rapidly than a consistently reinforced group. Are we led at once to the conclusion that different processes of learning operate in fish and rat? Certainly not. We must consider carefully the possibility that the relation between consistency of reinforcement and resistance to extinction is influenced by certain contextual variables, such as drive level or effortfulness of response, and that in our experiment with the fish we have permitted one of these variables to take on a value well beyond the range prevailing in experiments with the rat. Interactive interpretation of this kind can be tested

without great difficulty. It does not matter that we are unable to equate such variables as drive level in fish and rat; we know how to manipulate them and that is enough. If, working with either animal, we find the same relation between consistency of reinforcement and resistance to extinction at diverse levels of drive, the possibility that a difference between animals may be explained in terms of drive level is ruled out, and other interactive hypotheses can be tested in like manner. It may be well to note that the example which I have chosen is more than hypothetical. The initial resistance to extinction of Tilapia macrocephala (the fish with which my colleagues and I have been working) is greater after consistent than after partial reinforcement, and the outcome is the same with spaced or with massed trials, after relatively small or relatively large amounts of training, and at different levels of drive (Longo & Bitterman, 1960; Wodinsky & Bitterman, 1959, 1960). My main concern here, however, is with general strategy.

Given the decision to study some simple animal (such as the fish) under conditions analogous to those which have been used for the study of learning in the rat, with functional relations to provide the basis for comparing the two forms, there remains the question of where to begin. Intuitively, there seem to be many points at which the learning of fish and rat might be compared with profit, and my only verbalizable guiding principle has been to begin with conditions whose effects at the level of the rat have resisted analysis in terms of our simplest constructs. Contemporary learning theory seems to me to underestimate the rat, but I have developed a certain sentimental attachment to it, and the idea has occurred to me that a more comfortable place might be found for it at some earlier place in the animal series. That is, the rat phenomena which embarrass the theory simply may not appear in some more primitive form. Consider again the paradoxical effect of partial reinforcement on resistance to extinction in the rat, which once it seemed possible to explain in terms of stimulus generalization. The neo-Guthrians have attempted to bolster the failing generalization principle with a rather dubious habituation principle, but even the two principles together cannot deal adequately with all of the data. Suppose, however, that partially reinforced fish showed the paradoxical effect when and only when they could be expected to do so on the basis of these simple principles. We might then be willing to conclude that contemporary S-R theory is appropriate at the level of the fish, although new processes of learning came into operation at the level of the rat. As it happens, the paradoxical effect does not appear in our fish even where these simple principles suggest that it should; but I am only half serious, of course, about the possibility of finding an exemplary S-R creature in the lower reaches of the phylogenetic scale. I do, however, have considerable confidence in the notion that the mammalian phenomena which confound the theory are less likely to appear in more primitive species than are those which have suggested its basic postulates, and I have chosen to begin with the former because I am interested in maximizing the probability that functional differences will be discovered if they do in fact exist.

If the much-studied rat is to provide a phylogenetic frame of reference for comparative work with other animals, the choice of starting point also must depend on the structure of existing literature on the rat. Despite the volume of that literature, it is not exactly rich in well-defined functional relations, probably because so much of our past effort has been devoted to a search for crucial experiments designed to resolve certain very broad theoretical problems which arose out of the very earliest work on animal intelligence. I certainly do not regret this effort. The problems are real ones, and we have gained much insight into them. It is significant, I think, that nothing much in the way of

well-defined functional relations has been forthcoming even from those who have rejected the traditional questions and advocated a vacant empiricism; in the hands of the Skinnerians, for example, batteries of expensive automatic equipment have yielded little more than an idiosyncratic assortment of kymograph tracings scarcely capable of quantitative analysis. Theoretical concern may not lead of necessity to the plotting of functional relations, but certainly it must not be thought to constitute an inevitable bar. The position is easier to defend that a mature investigator does not take the trouble to make systematic measurements unless they promise to clarify some larger problem. In any event, the theoretical concern which I am here attempting to delineate encourages the discovery of functional relations--both in new animals and in old. I must note that my interest in comparative work with new animals has not made it possible for me to give up work with the rat. My original notion--that I would study only the new animals and compare the functional relations obtained with those already available in the literature for the rat--proved far too simple. Sometimes an experiment with the fish gives rise to questions about the rat for which there are no adequate answers in the literature, and I may be too interested in those answers to be content to wait until somebody else happens to supply them.

Exploratory work with new animals is not for the impatient; the ratio of achievement to effort, at least in the beginning, is rather small, which should not be surprising in the light of the history of research on the rat. One must learn how to keep each new animal in the laboratory, how to motivate it, and something about its perceptual and motor capacities, before the quest for an appropriate set of experimental conditions even can be begun. The difficulties are many, and failure much more common than success.

In our work with the fish, as it happens, my colleagues and I have progressed at a quite satisfactory rate. We have succeeded in developing a set of efficient, objective techniques, well suited to a variety of species, which permit us to attack the problems of learning in fish on a broad front. One of these techniques, which has been mentioned already, and to which we have given most of our attention thus far, involves the presentation of a target at which the animal is trained to strike for food reward. After some preliminary work with a crude, mechanical system (Haralson & Bitterman, 1950), we developed a more sensitive and reliable electronic one (Longo & Bitterman, 1959). The present target is a disk of metal mounted on a light rod which is inserted into the needle holder of a crystal phonograph cartridge, and the amplified output of the cartridge is used to operate a set of relays which record and reward response. A single-target apparatus may be used either in Thorndikian or in Skinnerian fashion. That is, one may measure the latency of response in discrete trials, each trial beginning with the introduction of the target and terminating with its removal; or one may measure rate of response to a continuously available target. With two targets introduced simultaneously, choice may be measured, as in the T maze or jumping apparatus. More recently, we have developed two additional techniques, one for the study of escape and avoidance, which is so closely patterned after Warner's shuttle box as to require no further description here, and one for the study of classical conditioning (Horner, Longo, & Bitterman, 1960a, 1960b). In our classical conditioning situation, the US is brief shock, and a paddle inserted into the water detects the generalized response which the CS soon begins to elicit. Again a phonograph cartridge plays an important role, its amplified and integrated output driving a counter which provides an objective measure of response-magnitude. The scope of comparative research made possible by these three techniques is considerable, but they came,

it must be emphasized, only after several years of trial which yielded very little in the way of useful data. Our work with other animals often has progressed at a much less satisfactory rate, and in some cases we have made almost no progress at all.

An invertebrate to which we have devoted a good deal of fruitless effort is the Bermuda land crab, Gecarcinus lateralis, which is quite easy to keep in the laboratory. It lives at 78° F. on some moist sand in a small fish tank, and it does nicely on half a peanut, some lettuce, a bit of raw carrot, and piece of eggshell once each week. It even does nicely for months on no food at all, which was a source of some disappointment to us, since we had been led to believe that its appetite was good. Failing at first to elicit consistent interest in food under our experimental conditions, we turned to escape. We tried light and heat which did not prove suitable, and then shock, which proved quite disorienting, often cuasing the animal to drop most of its limbs. At last, after some months of fruitless effort, we hit upon immersion in distilled water, which (probably because of its interference with salt regulation) seemed to produce a rather sustained effort to escape.

The uppermost drawing in Figure 3 illustrate the earliest form of the apparatus which we then proceeded to develop, an adaptation of a long, narrow fish tank that we happened to have on hand. We painted one end black, the other end white, and made a clear plastic starting compartment for the center, with two yoked guillotine doors that were raised simultaneously to permit choice. A coarse wire mesh ramp at one end led up out of the water to the home cage of the animal, also equipped with a guillotine door. The other end offered no escape from the water, but a dummy cage was set there for visual balance. In this situation, our crabs rapidly developed a preference for the positive side, whereupon we constructed the more elaborate apparatus sketched in the central portion of Figure 3. There was a stem leading to a T-shaped choice-point, and there were ramps on both sides, each of which could be blocked off by a barrier which was not visible from the choice-point. In this apparatus, the crabs did not learn at all; they would remain for long period in the stem, scrabbling ineffectively at the front wall. When we removed the stem and installed a starting compartment of the earlier kind, performance improved, but the elbows still seemed to make for considerable confusion. At last, we removed the end-sections of the apparatus, returning to the original linear pattern, and the animals regained their earlier efficiency. Several months were lost in making the circle. The new apparatus, sketched in the lower portion of Figure 3, does, of course, have features that the earliest did not. There are two ramps, with a guillotine door before each. Both doors are down to begin with, and the door on the correct side is raised only after the subject has made a correct choice.

Under these conditions, it was possible for us to complete our first formal study of learning in the crab--an experiment on habit reversal (Datta, Milstein, & Bitterman, 1960). In the course of that work, however, we had occasion to become dissatisfied with our technique. Clearly, we were able to achieve considerable control over the behavior of our animals, but the relatively low accuracy of our control group (which was not reversed) suggested the possibility that a higher level of drive should be sought. Now we are using dilute solutions of acetic acid in place of the distilled water. The acid strikingly improves both speed and accuracy of performance, but the animals do not survive many experimental sessions. Perhaps we shall be able to find a concentration strong enough to motivate the animals satisfactorily yet weak enough that it will not impair their health, or perhaps we shall be able to find a buffering

procedure to promote post-experimental recovery from the effects of immersion in strong solutions. So the search for a suitable motivating technique continues.

Another arthropod with which we have been working recently is the blowfly, Phormia regina. Our interest in this animal was stimulated by the physiological investigations of V. G. Dethier (for example, Dethier & Bodenstein, 1958), who taught us how to breed it and how to keep it in the laboratory. We have studied the fly in a simple runway and in a number of choice-situations, such as that shown in Figure 4. The work is done with harnessed individuals. The subject is anesthetized with CO_2, and a leash of light thread with a loop in its distal end is fixed to the dorsal surface with a bit of wax. The operation does not impair flying ability, but flight is limited in the experimental situation by a length of wire threaded through the loop at the end of the leash. In the apparatus sketched in Figure 4, the wire is curved, permitting the animal to alight only on one or the other of the two targets, one of which is baited with sugar solution. After innumerable variations of method and procedure, our efforts to develop situations suitable for systematic studies of learning in the blowfly seem finally to be meeting with a certain degree of success.

An animal with which we have had no success whatsoever as yet is the earthworm. For a long time I have been wanting to make a systematic study of the process of classical conditioning in a really primitive animal, and for the kind of work I have in mind it is not enough merely to look at the animal and try to decide whether or not it twitches when the CS is presented; an objective measure of response-magnitude is required. A few years ago, I came upon a paper by Galambos (1939) which seemed to meet the need. Galambos was interested in the movement of the earthworm, not in its learning, but his work led me to explore the possibility of a Bechterevian apparatus for the worm, the most recent version of which is shown in Figure 5. The animal lies in a narrow, covered trough, in the floor of which two electrodes are set, two loops of linen thread having been sewn previously into the dorsal musculature--one anteriorly and one posteriorly. A thread runs from the posterior loop to a fixed post, and another thread runs from the anterior loop to a spring which is attached to a second post. The spring is weak enough to permit withdrawal in response to shock, but strong enough to encourage return to the basal position when shock terminates. A third thread runs from the anterior loop to a kymograph on which response is recorded, as shown in the tracing. In this situation, brief shock elicits a response that looks very much like the flexion which shock to the limb elicits in the dog, and a neutral stimulus paired with shock may elicit a weak copy of the same response. Usually, however, the CR is a much more subtle affair which, though clearly visible to the naked eye, may fail entirely to activate the recording pen. After a good deal of time and effort devoted to the development of this apparatus, I have decided reluctantly that it should be abandoned in favor of an electronic technique which promises greater sensitivity.

Having emphasized the difficulties and disappointments which seem to be inherent in this exploratory work with relatively unfamiliar animals, I must note that it affords a rather special kind of enjoyment--a kind of enjoyment that I began to taste as an undergraduate in Schneirla's laboratory, but that I found only rarely in my subsequent work, which was more in accord with the spirit of the times. For many years, the field of animal learning has been dominated by a controversial, deductive spirit. Most of us have acted as though we knew all about the learning process, and as though the only purpose of our

experiments was to demonstrate the validity of our convictions. The controversy was fun, of course, and to some extent even productive--we managed certainly to accumulate a considerable amount of data on learning in the rat--but in my recent dealings with fish, and fly, and crab, and worm, I have come again to a kind of research that is at once more satisfying and more productive. Its function is inquiry, not proof. When I ask about the effects of partial reinforcement on resistance to extinction in the fish, or about the course of habit reversal in the crab, I have not the slightest notion what the answer will be; I can only wait eagerly for the outcome of my experiments. I am not suggesting, of course, that the same attitude cannot be taken in work with the rat. I am suggesting only that it comes more readily in work with the primitive animals, which are so far removed phylogenetically from those which have been taken as models by the various parties to the controversy--the lower mammals, on the one hand, and, on the other, man. Broadening the phylogenetic base of our work will facilitate the broadening of our outlook, and perhaps one day we shall be able to approach even the higher forms in the same spirit of discovery.

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FIGURES

Fig. 1. Cumulative-frequency curves for pigeon, rat, and monkey (not necessarily in that order) trained on a multiple fixed-interval-fixed-ratio schedule of reinforcement. (From Skinner, 1956.)

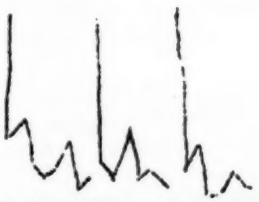

Fig. 2. Relative-error curves for sparrow, rat, and monkey (not necessarily in that order) trained in a Hampton Court maze. (After Small, 1901; Kinnaman, 1902; and Porter, 1904.)

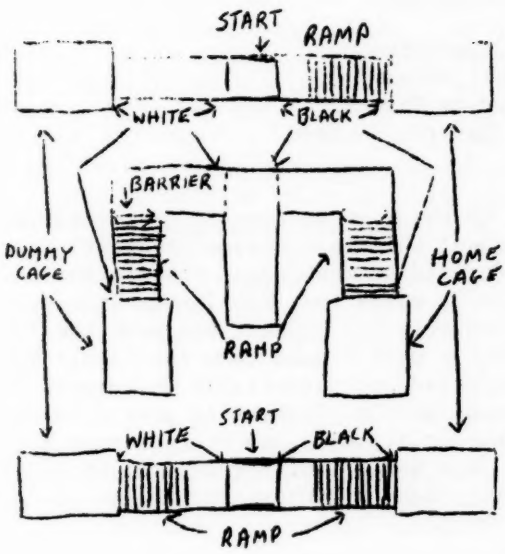

Fig. 3. Apparatus for the study of learning in the crab.

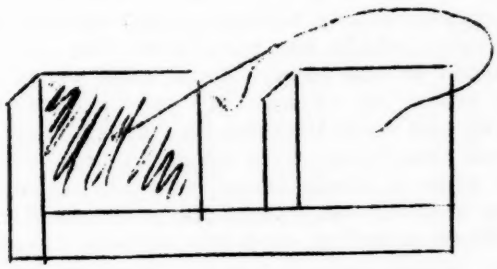

Fig. 4. Apparatus for the study of learning in the fly.

Fig. 5. Apparatus for the study of learning in the earthworm.

TITLE: Politic Worms*

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According to the Worm Runners' Digest (and let's have no giggling at the back there, please; this is a serious subject) experiments are now, right this minute, going forward at Washington University, St. Louis, that are enough to curl your hair. It seems that Washington University has a Dr. Edward Ernhart on its staff, and this Dr. Ernhart has made the fairly unattractive discovery that by splitting a worm's head down the middle you get not only, as you might expect, a maladjusted and potentially delinquent worm with a grudge against society in general and Dr. Ernhart in particular but a worm with two heads. (Dr. Ernhart doesn't actually say his patients are maladjusted after treatment but it seems a fair bet.) Furthermore this two-headed worm reacts more rapidly to electric shock-light stimulus than do the obsolescent Mark I worms with only one head. So there.

The deeper implications of all this only begin to writhe to the surface when we see that the Daily Telegraph, whence comes this awesome bulletin, describes the Worm Runners' Digest as a publication dealing with "studies started to find out if worms could be taught anything". Clearly there is more involved here than just good old Dr. Ernhart sitting up late at night with a candle, a couple of dry batteries and an old razor blade, chuckling to himself I have no doubt, while the pitiful mewing of timorous non-progressive worms rings unheeded in his ears. There must be an awful lot of people engaged in the higher education of worms if they need a whole publication to themselves. The majority are doubtless decent, sober family men, alive to their responsibilities and even kind, in a clinical sort of way, to the defenceless creatures whose fate lies in their hands; but one cannot help wondering how many unscrupulous worm runners have secretly progressed far beyond such baby stuff as electric shock stimulus, and are already cramming their exhausted charges with Beowulf and simple calculus until the poor overtaxed little brains are fairly reeling.

That dear old cosy the Mad Scientist, who crops up in the pages of extravert fiction with the persistence of a recurring decimal, has always been a favorite of mine, but it's a little disquieting to find nature copying art again and coming up with the worm runners. (I see them, Dr. Ernhart and his cabala, as slightly-built fanatics with crème pistache complexions and a bit of a twitch; in fact, now I come to think of it, closely resembling Alec Guinness in The Ladykillers--it was what you might call the definitive portrayal.) I admit a two-headed worm looks a shade puny beside man-eating giant crabs, carnivorous lichens quivering in the crypt and ravening homunculi in pickle-bottles; but I suppose Dr. Ernhart had to start somewhere.

Personally I have the gravest doubts about whether he should have started at all. It's not that I'm against worm education as such, and if there were

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one shred of evidence that this is what the worms themselves really want I should be the last to stand in their way; though what good a great horde of half-taught, disaffected and probably downright subversive worms is going to be, lurching about the countryside leaving anarchy and red ruin in its wake I do not know. But what I would ask Dr. Ernhart, in the sacred name of civilization and the future of the Race, is: What are these worms going to learn? To the more archaic technical skills--pottery, handweaving, simple village crafts of that sort--there would seem to be no objection; low-grade factory work is also a possibility, though precautions must be taken to ensure that no human artisan is deprived of his livelihood by avaricious employers cashing in on cheap worm labour. But can we be sure that it will end there? Once let those worms get started on political economy, and insurrection is only a matter of time; a couple of pages of John Stuart Mill and the jig is up. To say nothing of nuclear physics.

The very name these people have chosen for themselves implies an arrogance which can only arouse misgivings. Not Worm Watchers, not Worm Counsellors or Worm Guidance Officers, but Worm Runners. Kipling no doubt would have applauded; can we, in this day and age, afford to do the same? Up to now I daresay the tyrants have had things pretty much their own way; perhaps even jeered at their slaves for being so biddable, so pathetically anxious to please. But the one thing everybody knows about worms is that they turn; they turn, Dr. Ernhart; and where will you be then? Barricaded in your room I shouldn't wonder, clutching an empty insecticide gun and cringing with terror as the door panels bulge and split before the onslaught of your maddened ex-pupils.

Probably it's too late by now, but all the same I hope somebody is keeping a very, very strict eye on what those worms are reading.

TITLE: The Spatial Gradient of Alternation and Reactive Inhibition in the Earthworm[1]

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The recent controversy between Gelber (1) and Jensen (2), summarized and discussed within an historical perspective by Kellogg (3), regarding learning in paramecia, has raised anew the question of the quality and complexity of sub-mammalian behavioral modifications.

A number of studies have reported simple maze learning in various species of worms (4) and attempts have been made to study the occurrence of more complex learned behaviors in these organisms. Bharucha-Reid (5) has noted that earthworms learn a particular turning tendency in a T-maze more rapidly when confined to the maze for a period of time prior to the introduction of negative and positive reinforcements than do worms with no previous maze experience (latent learning); however, the possibility of some confounding in the experimental design due to methodological complications may have occurred (6).

In addition, meal worms as well as paramecia (7) have been shown to perform according to the reactive inhibition principle. These studies confirmed the hypothesis that an animal approaching a T choice along a path requiring a preceding turn to the left or right turned, at the choice, significantly in the opposite direction. Some doubt has been cast upon the phylogenetic generality of this principle by the subsequent finding that the behavior of Planaria dorotocephala, a species which falls between paramecia and meal worms on the phylogenetic scale, is not predictable on the basis of deductions made from this principle (8). Also, a recent paper has failed to confirm the findings with paramecia (9).

Since all of the studies of reactive inhibition in sub-mammalian species appear to have been based upon the same series of predictions from the reactive inhibition hypothesis, it was felt that some of the confusion may be eliminated by testing new experimental predictions from this hypothesis (10). Zeaman and Angell (11), studying the spatial gradient of alternation in albino rats, found that animals given a free choice in a four-goal fan-shaped maze after a series of forced trails to one goal show an increasing preference for the paths farthest from the path they had been forced to take. If the concept of reactive inhibition holds throughout the phylogenetic scale the spatial gradient found in rats should be evidenced by organisms at all levels of phylogenetic development. With slight modifications the investigation reported here replicates

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[1]Paper presented at the 52nd Annual Meeting of the Illinois Academy of Science, Illinois Institute of Technology, Chicago. April, 1959.

[2]On active duty at the U.S. Army Leadership Human Research Unit, Presidio of Monterey, California when this study was carried out.

the Zeaman and Angell study but utilizes earthworms, Lumbricus terrestris, as subjects.

The training apparatus was constructed by fastening wooden blocks to four plywood boards so as to form one training pattern on each board. Training pattern A had a 90° turn to the left; B, a 30° left turn; C, a 30° right turn; and D a 90° right turn. Each arm of the path was 9 in. long, 1/2 in. wide and 1/2 in. deep and was covered with Lucite to within 1 in. of the starting point. For the test trials a fan-shaped pattern with each of the four paths (A,B,C,D) available was constructed in the same way and with the same dimensions as the training patterns. Located at the end of the goal arm in both the training and test apparatus was a covered box 7 in. long, 2 in. wide and 2 in. high in which a thin layer of moistened earth was placed. For each worm the same box served as a goal box, "home cage," and to transport the worm. Illumination was provided on all trials by a 60-w bulb placed 4 ft. above the choice point. The maze floors were covered with strips of paper toweling moistened with tap water and changed at frequent intervals.

As in the study by Robinson (12) the Ss were housed in individual boxes: the same boxes, placed at the end of the goal arms, served as the "reward". During the training series the earthworm was removed from its box and placed in one of the four training paths: the Ss were given seven trials per day for eight days alternating every day so that each of the four mazes was used twice. The animal was allowed to remain in the goal box, thus avoiding the lighted runway, for four minutes on each trial.

On the ninth day the alternation tests were begun and carried out for twelve days. Each day's trials began with a free-choice in the fan-shaped maze in order to determine the earthworm's preference on that day. Twenty minutes after entering one of the goal boxes the earthworm was given ten forced trials in training pattern A or D: the particular pattern used was randomly changed from day to day although each was used for a total of six days. During these ten trials the earthworm was allowed to remain in the goal box for 30 sec. After the tenth trial each day the earthworm was again given a free-choice in the fan-shaped maze.

Fourteen earthworms, L. terrestris, ranging from five to seven inches in length were utilized in this study. Two Ss died in the course of this study and the data reported here are on the remaining twelve. Throughout this study a rubber-tipped probe was used to "motivate" the earthworms that halted before reaching the choice point: if the animal stopped at the choice point or in a goal-arm, a flashlight held perpendicular to the choice point was utilized (13).

The findings of this study may be noted briefly: employing the data analysis procedure of Zeaman and Angell (11) the results of this study are not in accord with the experimental prediction and thus afford no support to the hypothesis of reactive inhibition in L. terrestris.

This failure to find generality for an important theoretical construct of behavior theory (14) is not unique for the concept under study here. For example, in a series of studies (15, 16) on habit-reversal in fish (Tilapia macrocephala), a series of problems involving in part the compounding of after-effects, receptor-orientation and mediated distinctiveness of cues (16). there are marked differences from that behavior which would be predicted theoretically.

We may conclude then, that one cannot assume phylogenetic generality of learning theory concepts but must experimentally demonstrate their applicability to the various species on an individual basis.

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2. D. D. Jensen, Science 125, 191 (1957). See summary in D. D. Jensen, Science 126, 1341 (1957).

3. W. N. Kellogg, Science 127, 766 (1958).

4. See Kellogg, ibid., R. P. Bharucha-Reid, Science, 123, 222 (1956) and J. Arbit, Science, 126, 654 (1957) for references to this literature.

5. R. P. Bharucha-Reid, loc. cit.

6. J. Arbit, A failure to confirm "Latent learning in earthworms," unpublished manuscript.

7. J. H. Grosslight and W. Ticknor, J. comp. physiol. Psychol., 46, 35 (1953); W. Lepley and G. E. Rice, ibid. 45, 283 (1952).

8. G. E. Rice, Jr., and R. H. Lawless, J. comp. physiol. Psychol., 50, 105 (1957)

9. S. J. Lachman and J. M. Havlena, Photic reactions, reactive inhibition, and spiral behavior in the paramecium, paper read at the Midwestern Psychological Assoc. meetings, Detroit, May, 1958.

10. A recent abstract [(Psychological Abstracts 31, #7270 (1957)] notes the publication of a paper testing the reactive inhibition hypothesis in terms of the spontaneous alternation in a 3-choice T-maze of Armadillidum vulgare, a terrestrial isopod crustacean. This report [(M. Watanabe and S. Iwata, Annu. Anim. Psychol., Tokyo, 6, 75 (1956)] is unavailable to the present authors, however.

11. D. Zeaman and D. Angell, J. comp. physiol. Psychol., 46, 383 (1953).

12. J. S. Robinson, J. comp. physiol. Psychol., 46, 262 (1953).

13. Research now in progress at the Illinois Institute of Technology under the direction of P. S. Shurrager has employed an apparatus which appears to eliminate the need for these additional stimuli. A groove in the floor of the maze covered by paper toweling affords the worm a more natural crawling habitat through which it apparently moves with ease.

14. C. L. Hull, Principles of Behavior (D. Appleton-Century Co., N.Y., 1943).

15. J. Wodinsky and M. E. Bitterman, Amer. J. Psychol., 70, 569 (1957).

16. M. E. Bitterman, J. Wodinsky, D. K. Candland, Amer. J. Psychol., 71, 94 (1958).

TITLE: Intelligence and the Behavior of Fishes*

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To compare the "intelligence" of one animal with that of another implies that similar, if not identical, tests are used. Negative findings concerning performances based on sensory acuity and/or neuromuscular coordination have to be taken with caution. Often enough it turns out that not the animal, but the apparatus or method was at fault. That pigeons can learn to play ping pong with other pigeons, for instance, would have been doubted before the advent of the Skinner box. A case in point for fishes are the experiments with the minnow (Phoxinus) where the exclusive use of negative reinforcement led to pronouncements of a limited capacity for tone discrimination, later to be corrected by improved methods (Wohlfahrt versus Dijkgraaf and Verheijen, after Thorpe, 1956).

There are but few studies with fishes which meet the requirements for valid comparative tests (Bitterman, Wodinsky and Candland, 1958)--a deficiency which reflects the traditional interests of psychologists and the "this set-up works well with other animals, let's try it on fish" attitude of experimenters as well as the great difficulties of establishing identical performance tests with land and water animals.

Sensory components of the fish nervous system are highly discriminative, e.g. smell (Hasler, 1954), vision (von Frisch, 1924) and temperature perception (Bull, 1936).

The sensitivity of the sense cells is usually greater than seems to be warranted by the fluctuations of environmental variables in nature. Most sensory mechanisms work by summation, and full efficiency at the normal level of stimulation may well only be achieved with a threshold lower than that which would normally be used. Such low thresholds imply the existence of central processes which make for disregard of random "white noise" in most or in all sensory modalities, but they also imply that the animals can make choices with a high degree of accuracy and confidence of goal oriented success.

Many acts of discrimination or choice in nature are based on conditioning of the fish to single or multiple stimuli and even if no conditioned response experiments had ever been performed, there would be no doubt that fishes can learn. This ability, in the widest sense, is present even in simple invertebrate animals; but one can make a point for distinguishing relative levels of intelligence on the basis of the speed of learning and the retention span of learned responses, among other criteria.

Shock conditioning of goldfish (Carassius auratus) to visual stimuli and of bluntnosed minnows (Pimephelas promelas) to olfactory ones led to learning

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*Readers please note that this review was written before the appearance of the article by Bitterman (Amer. Psych. 15 (11) 1960) which is reprinted in this issue of the WRD.

accomplishments detected by autonomic reflexes (altered heartbeat) after 35 trials (McCleary, 1960) but fifty or more trials were necessary to establish a motor (swimming) response (Walker and Hasler, 1949). Great individual differences exist with some fish in an experimental group reportedly slower than others or even unable to learn a "simple" task. Here again, one suspects that not the task, but the teacher or the method may have been at fault though no one will deny the probability that fish intelligence follows a similar distribution curve to that of man. An apparatus, based on Skinnerian techniques, permitting the testing of learning abilities of fishes has recently been improved and described by Longo and Bitterman (1959).

Habituation and trial and error learning by fishes enter into some aspects of trout stream management (Miller, 1953, 1958). These streams have a native trout population but to satisfy the high fishing demands placed on them, large numbers of legal sized hatchery trout are introduced at the beginning of and also during the fishing season. At that time these may outnumber their native species mates. At the end of the season the native trout are again more numerous, the introduced animals having perished or been hooked in disproportionate numbers. After all, prior to their release in nature they had learned to come to a man when he appeared in their field of vision because he was the dispenser of food and not the holder of baited hooks, to be shunned like the plague or whatever equivalent there is for the plague in the trout-world. Although these results are in part also due to the occupancy of established territories by native trout, and to lack of natural selection for vigor in the hatchery population, it seems very likely that any avoidance learning in nature proceeds faster and with fewer trials than the earlier mentioned laboratory experiments would have suggested. Conditioning by reward (food) can be established quicker than by punishment; ten to twenty trials were sufficient, depending on the task, to accomplish visual responses in the minnow Phoxinus (von Frisch, 1913) or to have Tilapia macrocephala learn space reversal of food location (Bitterman, Wodinsky, and Candland, 1959). Hoar (1958) found downstream migrating salmon (Oncorhynchus) to learn "quickly" to swim a constant course in a circular channel. These responses were probably established speedily because the fish were in physiological readiness for ocean migration involving swimming in schools over very long distances. One or two trial learning may well be normal in the fishes' natural environment, but it is difficult to show unequivocally in aquaria.

To cite examples of elasmobranch, rather than teleost reflex learning, tiger sharks (Galeocerdo cuvieri) learned to strike a target and to run a staked-out path for a food reward (personal communication Wodinsky, 1960 and observed by myself) and bull sharks (Carcharinus leucas) associated sounds of 400 to 600 cps with food (Kritzler and Wood, in press).

A second order conditioned reflex was established by goldfish (Bardach, 1956) when the locomotory response of leaving an experimental chamber on shock, enforced by buzzer signals, was transferred to follow warming in a small region of the body. Over a hundred well-spaced trials were necessary to bring the fish to this accomplishment but even then considerable difficulty and many refractory subjects were encountered. The problems inherent in apparatus and method make it uncertain whether one may have reached the limit of this particular aspect of fish learning when the animal can establish, in an experimental situation, a train of several integrated 1st and one 2nd order consitioned response.

Observation of and experiments in fish behavior suggest that learning abilities play an important part in the proper function of instinctive behavior patterns and that individual components of integrated reflex chains rely on simple sign stimuli as releasers (Thorpe, 1956). For instance, where the young seek protection in the mouth of the parent (e.g., cichlid fishes) such parental care involves the capacity to respond to and to learn one of several rather than one species-specific color pattern.

Noble and Curtis (1939) brought Hemichromis to rear the young of Cichlasoma bifasciatum, another cichlid fish, but they also found that Hemichromis, after this experience, refused to protect its own young. The interaction between innate behavior and learned signals thus makes it likely that 2nd order trial and error learned reflex patterns have become components of behavior traits of fishes in nature.

Experiments of sensory discrimination which first and foremost test sensory acuity rely on generalization after a conditioned response has been established; that is, on the animal's ability to respond to a stimulus similar but not identical to that involved in the original training procedure.

Generalization for different values of brightness and hue as components of vision was easily accomplished by various species in several families of fishes (McCleary, 1960; Brett, 1957). Generalization of olfactory clues by minnows (Pimephelas) has been shown by Walker and Hasler (1949), to cite only a few of many such experiments.

Retention of learned responses without subsequent reinforcement, or the possession of good "memory" is another criterion of relative intelligence. Salmon (Oncorhynchus and Salmo) retain "imprinted" stream odors, learned during a crucial juvenile period, throughout their entire lives of 2 to 6 years or more. The final phase, and probably all other portions of their remarkable homing performance, is based on similar abilities (Hasler, 1960). In view of reported "forgetting" of laboratory established conditioned responses after 1 to 4 months, on the average (Thorpe, 1956), one may suggest that the strong appetitive reward in the form of the completed spawning act which the salmon obtains for remembering the home stream odor is a strong incentive for fish memory. In addition, homing implies not only a good memory but also "latent learning" because sections and choice points on the return journey are encountered in the reverse sense to that in which they were learned.

Only the higher mammals, man, the primates, rats, dogs, cats, and the dolphins have so far demonstrated the ability to generalize on a "conceptual" level, perhaps with the aid of a "symbolic memory". These highly anthropomorphic terms, denoting the type of "insight" we are most familiar with, are used here for want of better ones. The process is best illustrated by an ape who fits two sticks together, piles up boxes, employs a hook and a string or even a mirror to help him obtain food objects when they are outside his cage or otherwise out of his reach.

For fishes--and many other animals--one must approach this problem differently. Tetrapods have prehensile extremities and birds, while often less well endowed in this regard, have highly mobile heads, capable of much "manipulation" (remembering the reference to the ping pong playing pigeons). A fish's apprehension and conceptual use of spatial relations--for these attributes are involved in what we commonly call "insight"--must be detected and measured in other ways.

Apprehension of spatial phenomena and insight learning exists in gobies (Bathygobius soporator) and probably in many other fishes with comparable habits. The goby in question swims over submerged tidepools during high tide and later, when the water has receded it jumps from pool to pool with an uncanny aim and without seeing the next water area at the take-off point. Transplanted fishes which have not swum over the region and are placed in the pools at low tide refuse to jump, or when prodded violently will eventually jump in a disoriented way. After spending twelve hours--over night--and during high tide in a series of experimental pools the same individuals jumped directly to the larger pools. Effective orientation was retained up to forty days without additional "high tide" experience (Aronson, 1951, 1955).*

Delayed responses, successfully learned, have been taken as indication that the experimental subject in question is endowed with insight. Some minnows (Phoxinus), after having been allowed to swim freely through and become familiar with a three door chamber, were later required to reach a previously sighted food morsel by swimming forward and doubling back through one of two possible pathways to reach the reward which was then out of sight (Munn, 1958). Von Schiller (1949) had reported earlier that the minnows were successful in learning to solve delayed response problems. With elimination of external clues and refined experimental procedures it appeared, however, that many hundred trials did not improve the fishes' performance over an average of six out of ten correct solutions of the delayed reaction problem. The subjects also developed erratic and vicarious trial and error behavior at the point of choice as well as displacement activities such as picking at stray pieces of sand at the entrance into the channel which led to the point of decision. These observations suggest that stimulus satiation--that is, a negative "attitude" to the task or the experimental set-up formed after repeated experimental runs--may have bedeviled these experiments rather than their being an unequivocal demonstration that fishes are unable to learn to solve delayed response experiments.

How to test fishes for the equivalent to insight solutions involving manipulation is a thorny problem indeed. The more familiar one becomes with an animal species, the more one finds it can do. No one has ever "lived" with a fish, even in the sense that Lorenz (1952) lived with his jackdaws. The ethologists on the one hand (Tinbergen, Baerends, Morris, to mention a few who have worked with fishes especially) and scuba (self contained underwater breathing apparatus = aqualung) using biologists and observers on the other, like Cousteau, Randall and perhaps myself, have probably come closer to this state than anyone--but not close enough.

Field observations which corroborate Munn's (1958) findings, in spite of the earlier caution of stimulus satiation, were made in Bermuda on the escape of trapped snappers (Lutjanidae) and groupers (Serranidae). The fishes which had entered the traps would make a hundred or more trial and error attempts to find a way through the wire or netting before chance would help them to locate the exit out of the maze (Bardach and Sutcliffe, 1955). An hour's confinement did not lead to any attempts at what one might call systematic exploration. On the other hand, fishes of the same and other families (Centrarchidae) are known to make such traps their home and they will then swim in and out freely (Rodeheffer, 1945).

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*Recent personal communication (Mr. A. Witt, Dept. Zoology, Univ. Missouri) suggests that green sunfish (Lepomis cyanellus) resemble the goby in their homing behavior.

Another, perhaps more oblique, approach to the insight or conceptualizing ability of fishes was made by Bitterman, Wodinsky and Candland (1958) who compared rats to fish (Tilapia macrocephala) with regard to performance on inconsistent reinforcement of stimuli. On twenty trials per day, demanding visual discrimination between horizontal and vertical bars, responses to one stimulus were reinforced 70% and those to the other stimulus, 30% of the time. After 30 training days the previous 70% stimulus was consistently, i.e. 100% reinforced. Records of each day's response to the 70% stimulus showed that the rats "gambled" and that their preference for the more frequently reinforced stimulus rose gradually from a 70:30 level to reach 90% at day thirty.

The fish, however, after reaching the 70:30 level on about day ten, remained at this matching level of performance for the rest of the experiment.

Position training, demanding a choice between two positions of food placement, was conducted in a similar fashion and gave comparable results to those on visual choice. Sensory deficiency was ruled out as a cause for the difference in behavior between the mammal and the fish and it was suggested that the rat, in such test situations, makes an all or none response selection, an indication not only of a high level of versatility in cerebral processes, but also of a certain abstractive capacity. The fish, in contrast, matched continuously the probability of reward or punishment with the choice of its response and indicated, as in the case of the delayed reaction that it tends to perform in a more stereotyped and rigid manner than the mammal, in keeping with the more "limited" nervous system characteristic of this largest group of vertebrates.

SUMMARY

1) Few studies exist which either compare by means of simultaneous experiments the responses of fishes and other vertebrates or, if done on fish only, lend themselves to extrapolation of a comparative nature.

2) The learning capacity of fishes is well developed as shown by established conditioned reflexes of varying complexity.

3) Retention of learned responses depends on the condition of the learning situation; fishes can remember from several months to their entire lifespan, even of several years.

4) Generalization within one sense modality is easily performed and depends on an often highly developed sensory acuity.

5) Fishes are capable of insight--or--latent learning but delayed reaction experiments demanding generalization at a "conceptual" level or the type of insight that results in "gambling" have, so far, not been demonstrated to be within the capacity of the fishes' nervous system.

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TITLE: The Saga of "Project Glowworm" - Chapter III

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At the risk of making a pest of myself, hello again! You may have noticed (with a sigh of relief?) that there was no Project Glowworm report in the last issue. The study we were then in the midst of is finally finished, and I"m ready to tell you about it. (Of course, the boss twisted my arm a little.)

This episode describes an attempt to determine the effects, if any, that radiation has on the acquisition and retention of a classical conditioned response (i.e., the pairing of light with shock). We've found at our lab this last year that we can speed up the conditioning process with planaria by placing a small group of them in a single trough (up to 6 or 8 animals in a 10 inch trough) and exposing them to several hundred massed pairings of light and shock at one sitting. When individual testing in the conventional manner is instituted several days later, the subjects begin to show the conditioned response very quickly, frequently reaching criterion within 50 to 150 trials.

We made use of this information in the design of the present experiment by mass conditioning four groups of worms. Four animals from each group were then allowed to rest for ten days in the dark and given 250 individual conditioning trials over a nine day period. Three of the groups received 400 roentgens of x-irradiation at varying times in relation to the mass conditioning. The fourth group received no irradiation. This level of radiation dosage was chosen on the basis of previous experiments we'd done which indicated that it was safely below a dose which would cause any discernible radiation sickness, abnormality, or interference with regeneration. It was our suspicion that such radiation might interfere with the acquisition of a new response even though the animals appeared normal behaviorally.

Two months after the mass conditioning, four more worms from each group were individually conditioned in the same manner as the initial four. Here we might question whether the information gained from the mass conditioning was retained even by the control group, as well as whether such sub-lethal radiation had any long term effects on acquisition of the conditioned response.

Definition of the four groups in each time period and a summary of results obtained follows:

Group	Exposure	Delay	Exposure	Delay	Training
10 days					
I	mass condit.	2½ hrs	irradiation	10 days	indiv. cond.
II	irradiation	2½ hrs	mass condit.	10 days	indiv. cond.
III	irradiation	10 days	mass condit.	10 days	indiv. cond.
IV	no radiation	10 days	mass condit.	10 days	indiv. cond.
2 months					
I	mass condit.	2½ hrs.	irradiation	2 mos.	indiv. cond.
II	irradiation	2½ hrs.	mass condit.	2 mos.	indiv. cond.
III	irradiation	10 days	mass condit.	2 mos.	indiv. cond.
IV	no radiation	10 days	mass condit.	2 mos.	indiv. cond.

(See also graphs at end of article.)

I haven't run tests of significance on this data yet (it's hot off the apparatus), but I can tell you that within each group there was less variability than in most of the planaria data I've seen. Another way to break it which might profitably show trends is to compare the first 100 trials of individual testing with the last 150 trials. If this is done, the following pattern emerges:

Group	% R's 1st 100	% R's last 150	Group	% R's 1st 100	% R's last 150
I_{10}	33.5	63.0	I_2	27.0	25.0
II_{10}	29.7	67.5	II_2	25.3	21.7
III_{10}	48.3	49.0	III_2	32.3	29.8
IV_{10}	24.5	29.7	IV_2	47.8	41.5

Both the graphs and the comparisons of blocks indicate that groups I and II resemble each other very closely, both in the 10 day wait subjects and in those individually tested after two months. The 10 day and two month groups do not resemble each other, however. After 10 days, the animals appear to have retained little from the mass conditioning but are able to condition in the individual training. After two months these groups seem no longer able to condition, at least within a 250 trial training period. There is little evidence from the trend that they would have improved if carried further.

Group III appears to show some effects of mass conditioning after the 10 day period, though unable to improve its performance, even showing some evidence of extinction, in the individual training. After the two month period, Group III's overall performance is on a level higher than that of Groups I and II but lower than the Group III ten day animals. Group IV, which got no irradiation, is unusually low at the 10 day period, perhaps indicating some inhibitory effect of the mass conditioning. By 2 months after mass conditioning, this group performs better than any of the other groups, reaching its peak and dropping off rather earlier than the average conventionally conditioned animal.

To stick one's neck out and interpret this data, particularly before we've tested for significance, is downright brash. Should it be real, however, it is not inconsistent with our previous speculations on the nature of sub-lethal radiation damage. We suggested before that the subtle effects occurring might be in the direction of inhibition of neural cell growth or metabolic change. We might now hypothesize that the mass conditioning simply didn't "take" in Groups I and II due to its close proximity to the actual radiation, and interfere with the locking in of the response. These groups, then, after the 10 days had allowed dissipation of initial radiation effects, were able to learn the response from the individual training. We'd also have to presume that later, more permanent alteration of metabolic fluidity, had not yet set in. This is borne out by the fact that after two months, these groups could not learn, at least in the specified period.

Group III, which got its mass conditioning 10 days after irradiation, might be expected to retain the effect of the mass conditioning but be unable to learn as efficiently in the individual testing period occurring 10 days later. (Previc

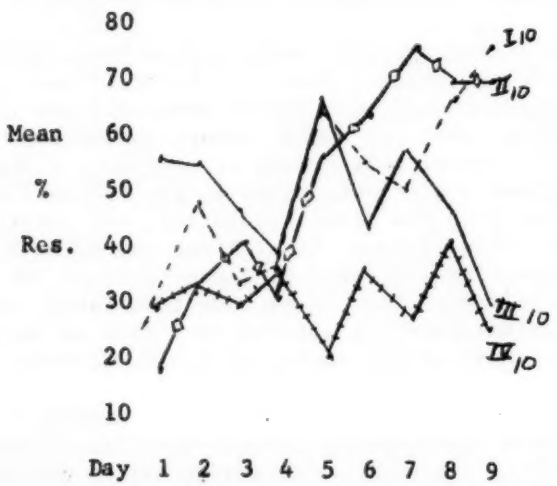
experimentation indicates that permanent damage, when it occurs, begins to manifest itself about two weeks after irradiation. This would be consistent with performance in the 10 day group (beginning individual training 20 days after radiation) starting comparatively high but showing no improvement, and after two months still being higher than I and II but showing a decrement (time?) from its own 10 day performance.

This brings us to the control group, which refuses to cooperate with our theorizing by being consistently lower than the other groups after 10 days. So we must ad hoc introduce another kind of inhibition, i.e. a sort of reactive inhibition caused by overexposure to the conditioned pairing in the 250 mass trials. Why this should occur at 10 days, when our previous experience with mass conditioning points to a facilitatory effect of mass conditioning for up to a week after its use, is anybody's guess. It is true, however, that our usual mass conditioning involves from 100 to 150 trials at a session while here we used 250 trials. Maybe this was too much for the control animals but just right for Group III who were in transition between temporary and permanent radiation effects. At least the control group took a firm lead over the other groups in the two month session.

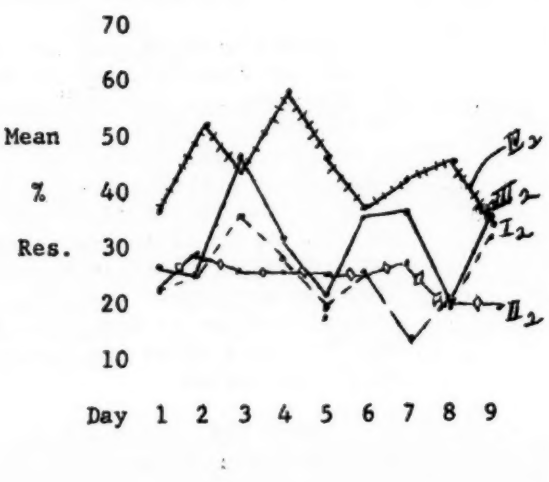
For the sake of my future reputation for scientific integrity, let me point out that in no other journal but this one would I speculate so rashly on so little evidence, or even submit it until it had been more firmly documented. But the Digest was born partly in a need of frustrated researchers to abreact in print, partly in the hope of encouraging speculative interaction and exchange of ideas while research is in progress. Both apply here.

Have I stimulated anyone to write and explain this data to me? Even your criticimms would be welcome--but watch your language!

Graph #1
Mean Percent Conditioned Responses Occurring in 9 Day Individual Testing for Groups Tested 10 Days after Mass Conditioning



Graph #2
Mean Percent Conditioned Responses Occurring in 9 Day Individual Testing for Groups Tested 2 Months after Mass Conditioning



TITLE: Maze Behavior of Planaria

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Although the experiments of McConnell and coworkers indicate that planaria are capable of type 1 conditioning, their capacity for type 2 conditioning has remained ambiguous in spite of several studies directed to this question.

The present studies were conducted to ascertain whether planaria are capable of learning a simple two choice maze situation when removal of water serves as the motivation to "run" the maze, restoration of water as the reinforcement, and light and darkness as the cues. The subjects (S) were of a species which has been tentatively identified as C. foremani. Twenty-two Ss were tested in the △ and Y maze situations, 4 in the rim maze situation which are described below.

Two types of mazes were used which shall be referred to as (1) the △ maze, (2) the Y maze. The △ maze consisted of three symmetrically spaced cylindrical wells in a block of clear lucite. The wells were connected with one another by three tunnels forming an equilateral triangle with the wells at the apices. Thus, each well had two tunnel entrances. During testing the maze was placed on an illuminator so any one of the tunnels could be illuminated. Since the floors of the wells and tunnels were all at the same level the entire system could be drained or flooded from any of the three wells.

The Y maze also consisted of three symmetrically spaced cylindrical wells in a block of clear lucite; however, in this maze the wells were connected by a tunnel system in the form of a symmetric Y. Thus each well had only one tunnel entrance and the juncture of the Y formed the choice point. The illuminator for the Y maze was constructed so that the wells could independently be placed in a light, neutral, or dark illumination state. The tunnels of the Y were flattened in cross section so the S had to approach the choice point with the plane of its body in a horizontal position.

Because the symmetric character of both mazes permitted any well to be the starting box for a trial, it was unnecessary to handle the S between trials. At the beginning of a trial set the S was placed in the maze with water. Once placed in the maze the S was allowed to remain in it through a trial set.

At the outset of a trial one of the alternatives was made light and the other dark (light and dark cues were randomized with respect to right-left position in every trial). The water was removed from the maze. Choice of the correct alternative was reinforced by flooding the maze with water. Water was withheld on choice of the wrong alternative. Some Ss were reinforced on the lighted alternative, others on the dark alternative. Trials were spaced about 10 minutes apart. Trial sets were spaced 1-3 days apart. 10 to 20 trials were

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administered per set. No difference was observed in the results on the two mazes.

The partition between light and dark choices on the first trial set did not differ significantly from 0.5. Both light and dark reinforced Ss exhibited a significant increase in the proportion of responses on the reinforced alternative in the second trial set. A comparison of the difference in the changes in the proportion of light choices from the first to second trial set between the light and dark reinforced Ss gives $P < 0.001$ on a t test.

Following this initial phase of enhanced probability of response on the reinforced alternative then ensued an unexpected phenomenon. On the trial set immediately following that in which the maximum proportion of choices of the correct alternative was observed, the S would do worse than chance, i.e. the proportion of choices of the correct alternative was significantly less than 0.5 and significantly less than on the first trial set.

The significant initial rise in the proportion of correct choices, both in those reinforced on the lighted and those reinforced on the darkened alternative clearly demonstrate an initial phase of learning. The precipitious subsequent decline in the number of correct choices cannot, moreover, be regarded as "forgetting".

A trial set or two following this abrupt reversal in choice behavior the S would become very lethargic in the maze and very difficult to test at all. Yet about four fifths of these individuals exhibited normal activity in their home bowl both immediately prior to and after being in the maze, indicating that the lethargy does not devolve from a physiological fatigue or injury state.

Some ancillary observations may be relevant to the reversal in choice performance and subsequent lethargy. An attempt was made to feed small pieces of raw liver to naive fasted planarians in the flooded maze wells. They refused to eat in the maze wells. No chemical property of the plastic was responsible since Ss fasted the same length of time would eat readily in larger trays made of the same lucite. If, however, the liver was placed in the maze well and the maze placed down in the Ss home bowl so that the water level extended over the top of the maze block (giving the Ss free entry to and egress from the maze wells) the Ss would climb into the maze wells and eat.

These observations suggested entrapment within the limited space of the maze wells, or the maze system, might be aversive to the Ss in addition to the effect of withdrawing the water during a trial. Thus perhaps the experimental situation entailed escape avoidance learning superimposed upon a generally aversive background situation.

In line with this conjecture another type of maze was constructed that had a well-tunnel system identical with that of the Y maze but which had, in addition, a rim extending up approximately 1.2 cm above the perimeter of the maze block. When flooded this provided an additional larger chamber into which the S could climb up into from the maze wells. When the water level was lowered at the onset of a trial the S would retreat into one of the maze wells and could then be run in the same manner used for the conventional Y maze. Two procedural modifications were introduced in the use of this maze. The intertrial interval was increased, the number of trials/set was reduced by a half,

a trial set was administered every day, and, for any given S, the right-left position of the light-dark cue was kept the same in each trial. Between trials, i.e. during the inter-trial interval, the upper chamber was flooded. Four Ss were tested in this manner, two being reinforced on the lighted alternative and two on the darkened alternative. The proportion of correct choices started out at a value not significantly different from 0.5 then rose to 0.7 which was maintained through about 10 trial sets (several times as long as previously) then declined to a value not significantly different from 0.5. Through more than two weeks of trial sessions no lethargic state ensued and the decline in proportion of correct observations, when it occurred, was not abrupt and did not exhibit the reversal to the incorrect alternative observed in the previous experiments. It is also noteworthy that the Ss did utilize the larger upper chamber in most of the inter-trial periods.

TITLE: Retention of a Conditioned Response Following Regeneration in the Planarian*

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McConnell, Jacobson and Kimble (5) found that a conditioned response could be retained in the planarian cutting and regeneration. In their study an increase in illumination served as the CS. The present study was undertaken to determine if the CR could be retained through regeneration if a different CS was employed. Part I of this study was conducted during the summer of 1959, Part II during the summer of 1960.

Part I

A classical conditioning paradigm was employed in which the CS was a decrease in illumination and the UCS was a mild electric shock just intense enough to evoke a consistent contraction. The animals (12 naive Dugesia dorotocephala, obtained locally) were run in a shallow, narrow, V-shaped plastic trough which had brass electrodes mounted at each end. The electrodes were connected to an inductorium which provided fluctuating 6-v. DC stimulation. The general room illumination was provided by two fluorescent lights placed behind and to one side of the trough at a height of about 6 ft. A small 32-w. fluorescent lamp was placed roughly 6 in. above the trough. At the beginning of each trial, the lamp was extinguished and the trough was then in relative darkness. This darkness lasted for 3 sec. During the final 1 sec., the animals were given the electric shock. At the end of the trial, the lamp was turned on again and remained on until the beginning of the next trial. Only those responses which the animal gave during the initial 2 sec. of darkness prior to the onset of the UCS (shock) were recorded. An animal was considered to have reached the criterion of conditioning if it gave 23 CRs during any 25 consecutive trials. A maximum of 30 trials were given each animal per day, with the minimum inter-trial interval being 1 min. Each animal was run until it had either reached the criterion of conditioning or had failed to reach that criterion in 390 trials.

Each animal which reached criterion was immediately cut in half transversely at about the level of its pharynx. The head and tail sections were then put in separate bowls and allowed to regenerate for approximately 26 days. After regeneration was complete, both head and tail regenerates were retrained to the original criterion in order to measure any savings of the CR.

Results: As Table 1 shows, both head and tail sections of each of the 7 animals which reached criterion evidenced savings of the CR. Using a t-test for matched samples, mean savings for the head regenerates is significant at the .05 level of confidence, that for the tail regenerates is significant at the .01 level. The mean number of reconditioning trials needed for the tails was considerably less than that for the heads, this difference just failing to reach the .05 level of significance. Five additional animals used failed

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*Research reported in this study was supported by grants to Prof. James V. McConnell from the Atomic Energy Commission and the National Institute of Mental Health.

to reach the initial criterion of conditioning within 390 trials. Although these five animals were discarded, most of them were responding noticeably more often to the CS at the end of their training than they had at the beginning of the experiment. Possibly all 5 might have reached criterion had not the limit arbitrarily been set at 390 trials.

Table 1

Number of trials needed to reach criterion of conditioning (23 CRs in any 25 consecutive trials) before and after cutting and regeneration for 7 planaria

Animal	Initial Trials to Criterion	Reconditioning Trials to Criterion	
		Heads	Tails
1	388	283	25
2	70	55	24
3	341	221	24
4	355	226	85
5	197	103	149
6	279	55	28
7	294	92	55
Means	275	148	58
SD*	101	86	44

*SD = standard deviation

Discussion: The above results are in substantial agreement with those reported by McConnell, Jacobson and Kimble (5). The greater savings shown by the regenerated tails was somewhat unexpected, however. It is possible that differing experiences during regeneration may account for this difference between retention of the CR by the heads and tails. All the animals were exposed to a considerable number of random changes in illumination while they were regenerating. During the early stages of the regeneration, however, the head sections possessed eyespots while the tails did not. Since the eyespots seem to be the chief photic receptors in the planarian (3, 4, 6), it could be argued that during the first few days after cutting, the heads could have responded to the shifts in illumination while the tails could not have done so until they had grown eyespots. Since responses to random changes in illumination would be the same as extinction trials, the heads would thus have had more opportunity to extinguish the CR than would the tails.

If the preceding line of reasoning is valid, we would expect that heads and tails which regenerated in complete darkness (or under constant and unvarying light) would show little difference in retention of the CR. Also, we would expect that the difference between the tails and the heads would be enhanced if we presented a continuing pattern of sudden changes in illumination to both sections during the early stages of regeneration, then kept both sections in unchanging conditions of illumination after the tail sections had grown eyespots.

Part II

Animals were run to test the above speculations, using controlled light conditions during the regeneration period. The conditioning apparatus was slightly changed, a U-shaped rather than a V-shaped trough being used. Since

we were interested in only the relative retention of head and tail regenerates under the various regeneration conditions, a modification was made in the procedure. Some preliminary data (see 2) seemed to show that conditioning is rather rapid in terms of experimenter time if a group of Ss is given massed conditioning trials first and later tested for conditioning. Using this modified procedure it is not possible to record individual responses during the group training session, and consequently the trials to criterion measure taken during the following individual training sessions is a maximum trials to criterion. That is, the S may very well have reached criterion during the group training period. Therefore, tests of retention following regeneration cannot be in terms of absolute savings for each animal. However, the relative savings of head and tail sections can be seen. If, in addition, it is assumed that the various regeneration groups reached their initial criterion in approximately the same number of trials it is then possible to make comparisons between groups treated differently during the regeneration period.

The modified procedure: A group of 4-8 Ss was placed in the experimental trough and given 150 classical conditioning trials (CS--light off, UCS--shock). These acquisition trials were run under massed training conditions, the intertrial interval ranging from 5 sec. to 15 sec. The trials were administered in blocks of 50, with a 5 min. interval separating each block. During this 5 min. interval the Ss remained in the trough, the fluorescent lamp remained on, and no electric shock was given. Since there were several Ss in the trough at a time no attempt was made to record responses. A 1-3 day delay period elapsed between the massed conditioning session and the start of spaced conditioning trials. The spaced conditioning trials were run in the same way as in Part I, with the criterion of conditioning remaining the same. During all inter-conditioning periods when Ss were not regenerating, the aquaria were kept in a darkened room, illuminated only by a small red light bulb. Red was used because a study of the light sensitivity of planaria showed them to be least sensitive toward the red end of the spectrum (4).

Upon reaching criterion Ss were allowed to feed on brine shrimp for a few hours. After 12-24 hours the worms were cut in half at the level of the pharynx, head and tail sections placed in separate bowls, and the sections given 10 days in which to regenerate. During the regeneration period three different extinction conditions were employed: 1) early phase extinction (Group E), 2) late phase extinction (Group L), and 3) minimal extinction (Group C). Group E received the extinction trials on days 1-4 of the regeneration period. Group L was given extinction trials on days 7-10 of the regeneration period. Group C received no extinction trials.

Extinction trials were given by placing the bowl containing the S in the extianction box. Two 7½-w. bulbs were placed in the top of this black wooden box, the lights being controlled by a micro-switch attached to a cam. The cam was driven by a small electric motor and was notched so as to cause the microswitch to shut off the lights during 3 sec. of each minute. Each animal being extinguished spent a total of 6 hours, or approximately 3600 trials, in the extinction box. No more than 2 hrs. of extinction trials were given on any one day. When not in the extinction box the regenerating Ss were kept in the darkened room, with a black box placed over the aquaria. Regenerating Ss were given brine shrimp once near the middle of the regeneration period.

Following the 10 day regeneration period Ss were tested for savings of the CR using the spaced trials conditioning procedure. Twenty-five trials were

administered per day. No more than 100 trials were given to any one S during this retraining period.

Experimenter bias: Two of the Es (GC and PC) administered the conditioning and test trials while a third E (MC) fed and cared for the animals, kept the master data sheets, and put a different code number on each aquarium each day. The code was not known to the two Es administering the conditioning trials. During much of the course of the experiment aquaria containing experimentally naive Ss were included along with aquaria containing Ss which had been given the massed training. During the period when previously conditioned and extinguished Ss were being run aquaria containing non-conditioned Ss were included with the aquaria containing the conditioned Ss. The Es administering the savings trials had no way of knowing the extinction history of the S, whether it had regenerated from a head or a tail section, or whether it had ever reached criterion before. By employing the above methods an attempt was made to keep experimenter bias at a very minimum. Inter-score agreement checks which were made periodically have been reported elsewhere (1).

Results: Table II shows that only in the no-extinction control group is savings of the CR apparent, there being little evidence for savings of the CR in the 6 extinguished Ss. However, the extinction condition variable is confounded with the maximum number of trials to the initial criterion. Ss in the control group had the highest mean trials to the initial criterion. Any evidence of savings by the control Ss might be due to their having had more conditioning trials. However, two experimental Ss (E-1 and E-3) received an almost comparable number of trials before reaching initial criterion and yet showed no evidence of retaining the CR.

Table 2

Number of trials needed to reach criterion of conditioning before and after cutting and regeneration. The effect of extinction trials during regeneration on savings.

Animal	Placement of Extinction Trials During Regeneration Period	Initial Trials to Criterion+	Reconditioning Trials to Criterion	
			Heads	Tails
C1	no extinction	98	67	75
C2	no extinction	121	failed*	28
E1	days 1-4	103	failed	failed
E2	days 1-4	24	82	failed
E3	days 1-4	75	failed	failed
L1	days 7-10	25	89	failed
L2	days 7-10	25	failed	failed
L3	days 7-10	25	failed	failed

+ trials to criterion in addition to the 150 massed trials given to a group of animals

* failed to reach criterion in 100 retraining trials

Discussion: Although the N is too small to draw any conclusions, it may be stated tentatively that extinction trials administered even as early in regeneration as the 1st to 4th days are effective in extinction of the CR in both head and tail sections. When no extinction trials are administered during regeneration there is a savings of the CR in both the head and tail sections.

The results of Part II offer no support for the hypothesis entertained in Part I, namely, that Ss regenerated from head sections should show less savings of a CR than Ss regenerated from tail sections because of the greater opportunity for the head sections to extinguish the CR during the regeneration period. Further experiments are needed to clarify the issues these two studies raise.

Summary

Fresh-water planaria can be conditioned to contract to decreased illumination. If a conditioned planarian is cut in half and both head and tail sections are allowed to regenerate, both halves will show savings of the initial CR, with the regenerated tail sections showing as much or more retention as the head sections. Using a small number of animals, different extinction conditions were employed during the regeneration period in an attempt to differentially influence the savings of the CR in head and tail regenerates. When no extinction trials were given during the regeneration period, both head and tail regenerates showed savings of the CR. Extinction trials given even as early as the 1st to 4th days of regeneration apparently reduce the savings of both head and tail regenerates considerably.

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TITLE: A Preliminary Report on Retention of Training Following Fission in Paramecia

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The data I have to tell you about were gathered almost a year ago and frightened me so that I put them aside until recently but I feel that I have no right to overlook them. We now plan a full scale study on the bits of information which seem to come from this exploratory study.

You may recall that in our usual procedure, we make cultures of about 512 paramecia which have largely consumed their food supply and stopped increasing for that reason. These cultures are exposed to a clean wire which is dunked into them, and they are practically always indifferent to that wire. Then it is smeared with a bit of food, very little indeed, and dunked into the culture. This is repeated several times on a predetermined schedule and the bugs are then exposed to the clean wire again. The number which cling to the wire are recorded as our data. Usually we have used 15 dunks of the wire with food on it.

In the present experiment, we thought we would borrow a trick from the worm runners and try something like a savings score, and see what would be the effect of autogamy which is, as you know, an individual kind of fertilization, involving nuclear restructuring, genic reassortment and so on, without mating. Well, we gave some of our cultures 5 dunks of the baited wire and compared their responses with other cultures which had no such experience. Neither kind of culture did very much and we decided that 5 trials just weren't enough. However, we did another thing. We started daily isolation lines from each of 2 kinds of cultures, educated (those which received 5 baited dunks of the wire), and ignorant (exactly like the educated in their biological history but which had never been near the wire at all). At the proper time, we started cultures from their progeny--just 1 paramecium to start each culture--so that when the cultures "cleared" or reached their maximal number (about 525 bugs in each), all of the paramecia would be autogamous, with the fragmented nuclei which are the most easily spotted symptom of this condition. We prepared other similar cultures so that they would be 1 fission past autogamy, at which time there are still some fragments of the old nucleus floating around in the cytoplasm. In several ways, the first fission is rather special, though I won't go into that here. Anyway, we gave both the autogamous cultures and the first fissions each 8 dunks of the baited wire. Of course, we also used control cultures everywhere. Now, this is the surprising thing that happened. While autogamous progeny of the educated did no better than the autogamous progeny of the ignorant, and both did not much better after half the amount of dunks we usually give, the other cultures which were one fission past autogamy when cleared, showed a difference at $P < .01$ in favor of the ones with an educated forebear. Not only that, but the educated in the first fission exceeded the educated which were autogamous, again beyond the .01 level of confidence--t test in both cases. Now, when you get that sort of result with only 3 cases in each group, you can't honestly decide that nothing happened but an unusual accident

unless you try it out a good deal more. We did some fooling around with this, and are now trying to get going on a really full scale experiment. One thing seems likely, and that is this thing does not happen after the pieces of the old macronucleus are resorbed. Anyway, that seems the best explanation of the fact that we tried twice to get a carry over from the 4th to the 8th or 10th fission and did not get it, when we thought it would be more likely to occur than in the first fission.

This is the story so far, puzzling but interesting. I have not tried to figure out an explanation because there is always the possibility that it was an unusual accident. We should know whether or not this is so within the next 6 months.

TITLE: The Effects of Ingestion of Conditioned Planaria on the Response Level of Naive Planaria: A Pilot Study (Or: "You Are What You Eat??")

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Conditioning in the planarian is maintained in spite of cutting and regeneration (McConnell, Jacobson, and Kimble, 1959). Savings have been noted in planaria regenerated from conditioned flatworms when the final subjects had never been exposed to the conditioning procedure (Jacobson, McConnell, and Maynard, 1959). These studies have led to an hypothesis that learning in the planarian causes some kind of relatively permanent change in the biochemistry of the animal. This hypothetical change would account for the transmission of learning during regeneration.

Grafting of a part of a conditioned worm to an unconditioned worm or injecting ground-up conditioned worms into naive worms has been attempted as a means of testing the chemical hypothesis. At present the techniques necessary for these procedures have not been perfected.

The planarian has a very simple digestive system (Hyman, 1951). The nature of this system makes possible the use of another technique for insuring the incorporation of the protein of a conditioned worm into the cells of a naive (untrained) worm. Since one species of planarian often eats another, it is possible to feed parts of conditioned worms to "naive" cannibals. Pieces of worms are drawn into the digestive system through the pharynx, a tube extending from the ventral surface of the worm. The digestive tract stores food which later is digested in individual cells. Substances ingested are not totally broken down until they are within the cell. The use of this "feeding" technique permits the study of the effects of ingestion of parts of conditioned planaria on the responses of cannibal worms.

Study I

Method

Subjects

Eight naive planaria of a cannibal species were used in this pilot study. They were obtained from various sources and stored in group bowls in a large room until tested. Four worms were fed a total of 13 pieces of conditioned planaria (Dugesia tigrina) while the other four cannibals were fed an equal number of naive planaria (Dugesia tigrina). The conditioned tigrina had all reached a criterion of 23 responses out of 25 consecutive trials and had been retested to criterion before being cut up to be eaten. All cannibals were fed approximately every four days before testing began. During the testing period all worms were offered food on December 3 and December 12, 4-5 hours before testing. On December 3 N1, N2, N3, and C4 refused the food; on December 12 C4 refused food and N1 was accidentally fed a conditioned worm.

Apparatus

The apparatus used in the conditioning of the tigrina and the testing of the cannibals has been completely described elsewhere (Jacobson, A., 1959; McConnell, Cornwell and Clay, 1960).

Procedure

The cannibal planaria who had ingested conditioned tigrina (Group C) and the cannibals who had consumed naive tigrina (Group N) were placed in individual bowls before the beginning of testing. They were moved into a dark room and remained there when not being run. Two worms were run at the same time, but each in an individual trough.

The conditioning procedure followed was that reported by Jacobson, McConnell, and Maynard (1959). It consisted of three seconds of light, with shock presented during the third second. The intertrial interval within a block of trials was approximately 30 seconds.

On the first and third days of training, 50 trials were given each day, with a rest between each block of 25 trials. On other days, Ss were run 25 trials per day. After every three consecutive days of training, the animals were allowed a day off. Worms were given more days off if sick. After the first 150 trials some of the cannibals became sick and refused to run, so data collection became sporadic and could not be completed.

Results

The data cannot be reasonably presented in terms of number of Ss in each group who reached criterion, for only two cannibals reached the predetermined criterion of 23 responses in 25 consecutive trials. Instead, the conditioned responses of each individual worm, for each block of 25 trials is presented graphically. Data are not recorded on graphs on the days when the cannibals were unable to run the full 25 trials.

Figure I presents the data on cannibal planaria who had ingested conditioned tigrina before and during the testing period. (See end of article for Figures.) All worms made more than 5 conditioned responses on every day tested. At 100 trials and beyond, all but one cannibal made 10 or more conditioned responses out of each set of 25 trials. The one cannibal (C3) who made only 5 responses on December 12 had, on all other blocks of trials, responded above the minimal level of the other worms. Cannibal C1 reached criterion in 154 trials. (Note: circled +4 means that this S had been given 29 trials in that block in order to achieve the 23 responses out of 25 consecutive trials required. In this experiment there was no carryover of trials from one day to the next. Instead, if an S approached the criterion, extra trials were given.) In the following block of 25 trials, C1 again reached criterion.

Figure II contains a graphic representation of the data on cannibal planaria who had eaten naive tigrina before and during the testing period. In the first 150 trials all worms obtained some scores below 5 responses. Cannibal N1 reached the criterion in 150 trials. However, on that day, about 5 hours before testing, it had accidentally been fed a piece of a conditioned worm. In the preceding 25 trials, this worm had made only 2 responses. On the following block of trials, N1 made 12 responses. Cannibal N3 was the only other worm in this group to make 20 or more responses. This level was achieved by all the worms in Group C.

The data presented in Figure III represent the mean responses of cannibal planaria on days when at least three worms out of the four in each group ran 25 trials. If no dot appears on a given day, this indicates that there was not enough data on those blocks of trials to average meaningfully. When the data are averaged, there is obviously no overlap between the curves of the response

rates of cannibals eating conditioned tigrina as compared with those cannibals who had ingested naive tigrina.

The cannibal study above was a pilot study; the number of subjects in each group was small and complete data was not obtained because of illness of some Ss. Therefore, the experimenters thought it imperative to repeat the study using more Ss and running these Ss completely blind to avoid any experimenter bias.

Study II

Method

Subjects

Twelve cannibal planaria were used in this study. They were separated into two groups; Group C had 23 feedings of conditioned tigrina before the beginning of training, while Group N had an equal number of feedings of naive tigrina. The only exception was C6, a regenerated tail from a cannibal who had eaten 19 parts of conditioned tigrina; C6 was fed conditioned tigrina four times before the beginning of training.

Apparatus

Same as Study I.

Procedure

The training procedure was the same as that used in the first study. In this study, however, records were kept showing when an S in Group C was fed the head of a conditioned tigrina rather than some other part of the body; also, Ss in this study were fed approximately 24 hours before testing.

The same experimenter who fed the worms also changed the numbers on individual bowls daily. Therefore, the experimenter who ran the Ss never knew which worms had been fed conditioned tigrina and which had been fed naive tigrina; it was impossible for the experimenter to know which worms were in either group or to compare scores of individual worms on successive days. In this way we hoped to control for experimenter bias.

Results

Table I presents data showing when, during the training period, a member of Group C ate the head of a conditioned tigrina.

December	C1	C2	C3	C4	C5	C6
12	--	--	--	--	H	H
16	--	--	--	H	--	--
20	--	--	--	H	--	--
25	½H	½H	--	--	--	--

The cannibals who were not fed heads were fed some other part of the conditioned tigrina on the same days.

Figure IV is a graph of the mean number of responses per day when at least four Ss received a full 25 trials. (A complete block of 25 trials was not administered when the subjects were sick and/or would not run.) At point (a) C5 reached a criterion of 23 out of 25 consecutive responses. There are no data plotted for the 15th for Group C since only three members of that group ran a full number of trials (point (b)). However, on that day C6 made 24 correct

responses out of 25 trials. At point (c) one member of Group N reached the criterion. He had received a total of 275 trials.

After a worm has reached criterion, the number of correct responses that he makes on the following blocks of trials decreases. (This is a general phenomenon observed in most planaria studies.) Therefore, it was suggested that the data in this study be graphed in the same way that rat data are usually plotted. That is, after a worm reached the predetermined criterion, his responses on all future days are recorded at the level he reached upon achieving criterion. Figure V is a graphic representation of the same data given in Figure IV, plotted in this modified manner. This makes the curve for Group C higher in many places than the curve for Group N. However, the curves still overlap. Since only three Ss out of twelve reached criterion and the curves for both groups overlap, clear differences, like those in Study I, are absent.

Discussion

The two studies which have been presented indicate a possible relationship between ingestion of conditioned tigrina and a higher number of correct responses in the cannibals who ate the conditioned worms. The studies are inconclusive at this stage, but the possibility of this relationship is suggested by a study by Corning (1960).

Corning reported differences in savings in regenerated head and tail sections of conditioned worms if the worms regenerated in RNA-ASE. He found that RNA-ASE seemed to cause a lack of retention in the regenerated tail section, but did not affect the savings demonstrated by the head. The tail had to regenerate a much larger part of its nervous system than did the head. If RNA is altered during learning, but reverts back to the"naive" state when broken down by RNA-ASE, it is possible (since the tail had to build so much new nervous tissue) that the rebuilding involved normal RNA rather than the changed RNA (RNA').

If the conditioning procedure causes a change in RNA (as suggested by Corning's work) this change would be as stable as is RNA. The change in the RNA of a conditioned planarianmay be part of the mechanism for transmission of learning from one area to another.

Due to the simplicity of the planaria's digestive system, the RNA' could be incorporated into naive animals by the ingestion of conditioned worms (worms which contain RNA'). This might be accomplished by whole cells from the ingested conditioned worm migrating from the digestive system into the mesenchyme layer of the cannibal. Or perhaps the ingested cells are broken down, but the RNA' in an unaltered state is absorbed into the cells of the cannibal. If the RNA' is incorporated into the body of a cannibal, it could influence the future production of RNA' in the "naive" cannibal. At this stage of our knowledge it is not possible to demonstrate the above hypotheses or to postulate a mechanism for changing RNA to RNA' during conditioning.

It must be remembered that these cannibal studies represent the very earliest stages in our work. The results should be interpreted as no more than a possible indication of a difference between two groups of cannibal worms who have been fed on special diets.

At present we are trying to grow large tigrina which will feed on conditioned or unconditioned members of the same original stock. In this way some of the variability which exists in the cannibal planaria would be eliminated,

and the results obtained would be more comparable to data from earlier studies on conditioning and transmission of learning.

Summary

In Study I, two groups of four cannibalistic planaria, and in Study II, two groups of cannibalistic planaria, were fed diets of parts of either conditioned or naive tigrina. The cannibals were then tested for responses in the first two seconds of light in a light-shock apparatus. In curves plotted for mean response rate in 25 trials, there was no overlap between groups in Study I. The curve for the cannibal planaria in Study I who had ingested conditioned tigrina was considerably higher than the curve for the cannibal planaria who had ingested the same amount of naive tigrina.

In Study II there was some overlap between the two groups, but the results were in the same direction.

These two studies are considered inconclusive and further studies are in progress.

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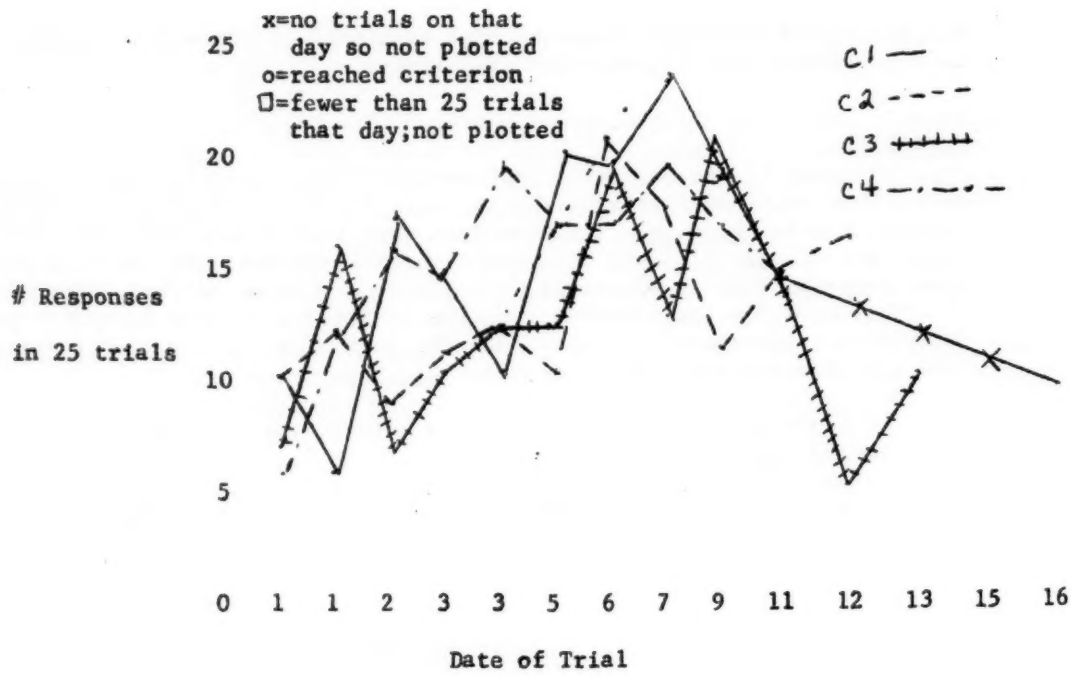

Fig. 1. Responses of cannibal planaria who had ingested conditioned tigrina.

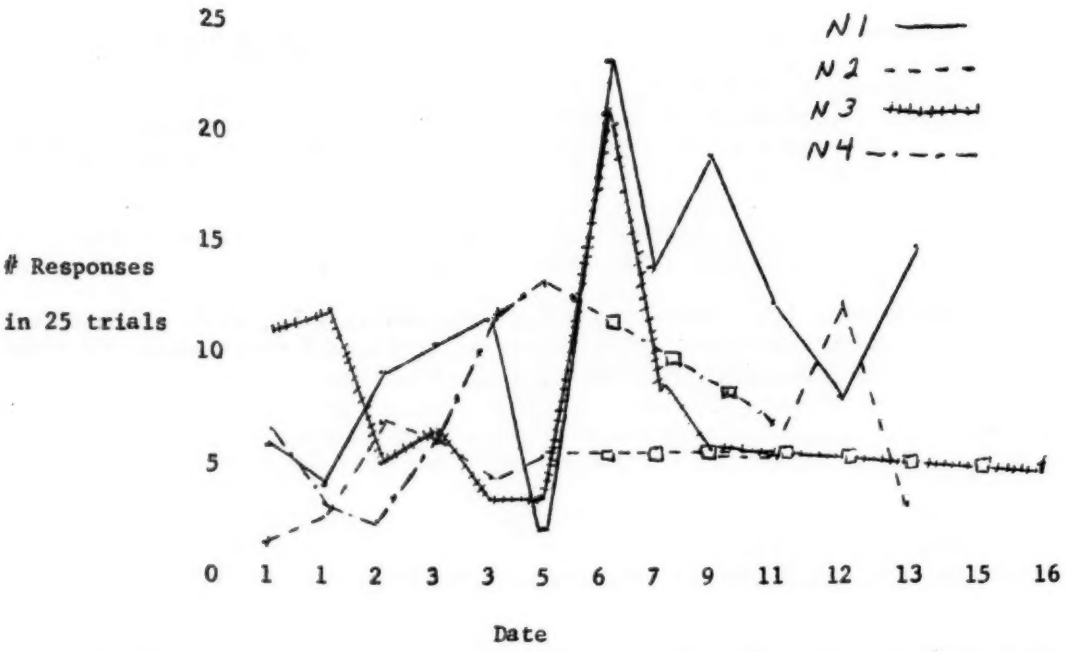

Fig. 2. Responses of cannibal planaria who had ingested naive tigrina.

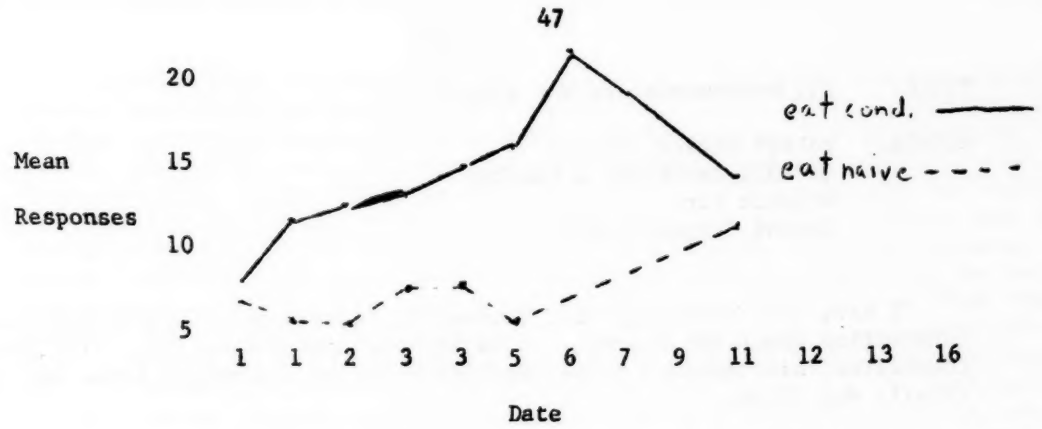

Fig. 3. Mean responses in each block of 25 trials for cannibals eating conditioned tigrina and cannibals eating naive tigrina

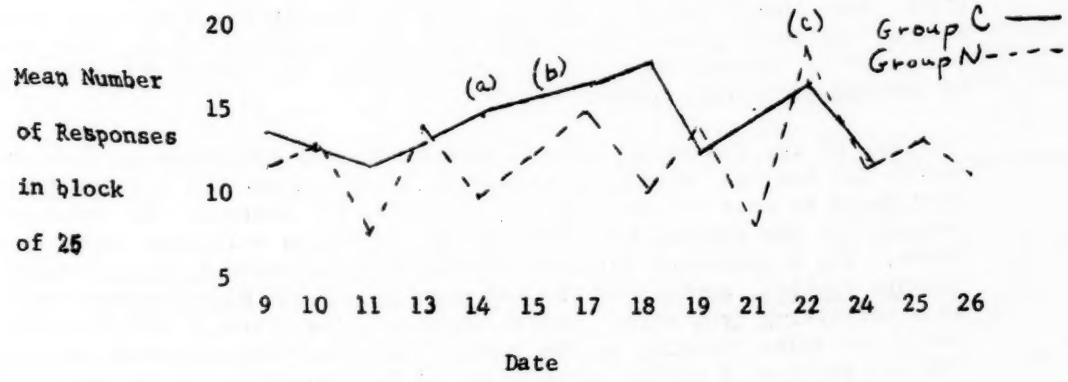

Fig. 4. Mean number of responses per day when at least 4 Ss received a full 25 trials

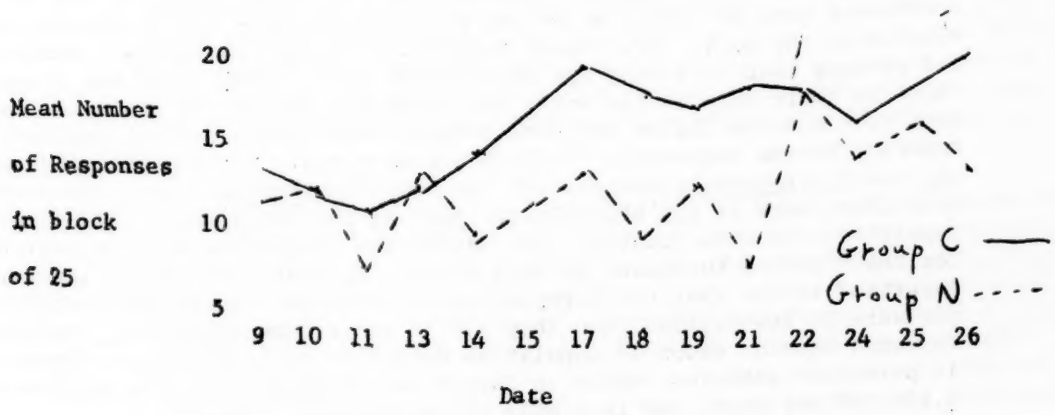

Fig. 5. Data from Fig. 4, plotted in a modified manner.

TITLE: All Earthworms Are Not Alike*

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I have just turned up what I consider to be some extremely fascinating information about earthworms. It is not new information, but, like the worms themselves, has remained buried and invisible to all except those who specifically dug it out.

Chiefly, this information concerns the different species and varieties of earthworms. It is remarkable how little the average gardener knows about the different types of earthworms that stock his soil. Farmers are likewise guilty of earthworm ignorance. A farmer will be very particular about the bloodlines of his cattle, but knows nothing of the parentage of his underground stock, even though worms in pasture land frequently weigh twice as much per acre as the above-ground stock that acre will support. We may know about "night crawlers" or "manure worms," but actually there are over 2,000 different types of earthworms on this planet.

One of the few really significant studies of earthworm species was conducted in New Zealand, and is described in detail in the new book Better Grassland Sward by Adre Voisin. It is, in truth, the story of the invasion and conquest of New Zealand by a few species of highly efficient European earthworms. The 3 principal soldiers in this invasion were Lumbricus rubellus and Eisenia foetida, members of the red worm group--and Allolobophora caliginosa, an enterprising grey worm. Before these worms arrived in the Southern Hemisphere, no doubt clinging to the roots of plants being imported, New Zealand had 110 species of native earthworms. A New Zealander by the name of F. W. Hutton counted and identified these native types back in 1876, and we should be forever in his debt.

According to Mr. Voisin's report, the 110 native types of New Zealand earthworms were not doing a 100 per cent satisfactory job of digging up and vitalizing the soil. Mr. Voisin refers to them as "aboriginal" earthworms, and perhaps that is a very apt descriptive word. They could get along all right in their thin native soils but, possibly because of their lack of contact with manured fields and dank compost heaps, they had not evolved into superior breeds comparable to the European worms. As soon as Lumbricus, Eisenia and Allolobophora stepped off the boat from England, they proceeded to make short work of the aboriginals, just as the white men decimated the Indian populations in this country. The native New Zealand worms were just no match for the vigorous Europeans in many areas. In fact, the New Zealanders were surprised to see that the European worms achieved even greater populations per acre in New Zealand than they did in their home countries. Rukuhia Soil Research Station recorded population densities of 4,300,000 earthworms per acre in permanent pastures, while in Europe the maximum is in the neighborhood of 1,200,000 per acre, and then only in exceptional cases.

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Now, the plot begins to thicken. There is a tradition in New Zealand, passed from father to son by word of mouth, that 50 or 100 years ago the soil of that country suddenly became more fertile without any fertilizer being added. The grass became greener and more luxurious and the yields of all crops went up. No one could be absolutely sure of the reason for the change, but many suspected that the European earthworms were doing something that was changing the soil. One farmer in the Raetihi district, Mr. A. S. Ashmore, decided to conduct an experiment that might pin down the reason for the soil improvement. His land was high in an isolated hill district into which the European worms had not yet penetrated. In 1925, he began a persistent program of implanting worms by digging up turfs from fertile valley soils populated by European worms and "planting" them on his hillsides. He continued his worm-moving project until 1945, and by that time Mr. Voisin reports that the results were striking. "The poor hill pastures that had formerly comprised brown top and Danthonia (a species found on poor soils) were now transformed into rye-grass-dominant swards," says Mr. Voisin. "Stocking rates and yields per acre had increased enormously parallel to this improvement in composition."

R. L. Nielson of the Rukuhia Soil Research Station came to Ashmore's farm in 1949 to check the results of his experiment. He paid particular attention to a section which had received the worm transplants only recently. This section had a very low population of Allolobophora calignosa, and the grass there still consisted of the old, poor types. It had only one-twentieth as much rye-grass, for example, as the fields with heavy European worm populations. Mr. Nielson concluded that worms were in truth responsible for the tremendous improvement in Mr. Ashmore's pastures.

Mr. Ashmore's experiment was a great contribution to our knowledge of the activity of earthworms, and it opens the door to an entirely new practical application of the science of zoology--the transplanting of earthworms from one part of the world to another in the hope of improving soil fertility. Actually, however, the experiment poses more questions than it answers. By what mechanism did Allolobophora calignosa improve Mr. Ashmore's pastures? Did it simply aerate the soil and mix humus from surface litter into the topsoil, or did it bring in its intestines some mysterious growth factor? Can other parts of the world benefit from earthworm implanting to an extent even remotely approaching the benefits to New Zealand? Can we crossbreed worms to create a super race of underground creatures that will so improve the fertility of the world's soils that our hunger problems will have ended? No one is sure of the answers to those questions, but everyone would agree that they are most challenging and offer the prospect of a whole new era of soil improvement.

Like New Zealand, the U. S. has also been a testing ground for superior breeds of earthworms. Many organic gardeners and farmers are familiar with the work of George Sheffield Oliver, particularly his book Our Friend, the Earthworm. And uncounted thousands of people have purchased descendants of the "hybrid" earthworms he claims to have developed out in La Canada, California, back in the early thirties. Here is the story of how they came into being. Oliver was very impressed with the vigor of the "brandling" type of worm that inhabited rotting manure piles. These small, bright-red worms multiply quickly and do a wonderful job of composting manure. They deposit their castings under the surface of the soil, where their concentrated nutrients are readily available to plant roots.

However, the brandling worm is active only in the upper 6 inches of soil. Oliver's idea was to cross the brandling with a variety of orchard worm that spent most of its time deep in the soil, and often went down 10 to 12 feet. To achieve the mating he placed one worm of each type in a series of containers, and when eggs were laid he collected them and placed them in incubating containers. From the offspring he selected about a thousand of the most vigorous and used them for continued breeding. To the new race he gave the name the Soilution worm. According to Oliver's reports the Soilution worm achieved all that he expected of it. Certainly it has been widely dispersed throughout the U. S. by worm breeders and organic earthworm enthusiasts.

Now, the question in my mind is whether Oliver's Soilution earthworm really is a hybrid, or is it still the original brandling worm? Brandling, incidentally, is the popular name for Eisenia foetida, the hard-working little manure worm that did such a fine job down in New Zealand. It is my opinion that Oliver did not succeed in creating a new hybrid species of earthworm, and I base this opinion on two lines of reasoning:

1. Oliver, in his writings, does not demonstrate proficiency in the taxonomic identification of earthworms. For example, he doesn't identify the species used to mate with the brandling, but just states that it is a "deep-working orchard worm". He does not describe the appearance of his hybrid or specify in what ways its anatomy differs from that of either of its parents.

2. According to my limited knowledge of zoology, crossbreeding between species is very rare. It is likely that the earthworms Oliver placed together to mate had already mated with members of their own species, and therefore carried in their bodies the sperm that enabled them to produce fertile eggs.

Oliver shouldn't be blamed if he failed to do what he set out to accomplish. He did succeed in focusing attention on the helpful qualities of earthworms, and the information in his book Our Friend, the Earthworm has enabled thousands of people to produce and use earthworms effectively. Oliver was a pioneer in the large-scale production of earthworms, and had worked out a system for natural food production before the organic method in its present form had been originated. My purpose in telling the story of his "hybrid" earthworm is to illustrate the need for more precise studies of the different types of worms and to investigate the practical benefits that might accrue from the selection and implanting of more efficient species.

It is a sad but true fact that our knowledge of earthworms is very meager. On November 28, 1951, Dr. G. E. Gates delivered a talk on earthworms over station WGY in Schenectady, N. Y., in which he stated that to the best of his knowledge there were only two people in the world engaged in collecting and identifying earthworms. One was a young man in the far Antipodes Islands and the other an American housewife, with a farm and children to occupy most of her time. It is probable that the situation has not changed much since 1951.

Coincidentally, Dr. Gates also spoke about the take-over of New Zealand and other areas by European earthworms, and mentioned that the same thing is probably going on in the U. S. He implied that some of the species of native American earthworms may never be found and identified, because of their extermination by imported worms. And he made the rather alarming statement that

"Earthworm farmers and organic gardeners by the thousands have been engaged for 10 years--quite unintentionally--in speeding up that process of extermination." Perhaps we organic gardeners have been the cause of wiping out some native earthworms, but in doing so we have increased the fertility of our soils. If we can exchange a few inferior species of earthworms for a better soil, we will have made a good bargain.

Trying to make practical use of earthworms without a sound knowledge of the identity of these creatures is not good policy. As far as I have been able to determine, the most recent book on earthworm identification was published in 1930--in England. How complete and useful it is, I don't know. My feeling is that since the earthworm is a friend and partner of the organic gardener, we organic people should shoulder the job of finding out more about him. We could start by taking a census of earthworm species in some of the major agricultural and gardening areas of the U. S. We could find out what kind of earthworms inhabit the most fertile soils. Even if nothing sensational were discovered, we would at least have laid the foundation for future studies.

It should not be difficult to make arrangements with one or more colleges or universities for the taking of this earthworm census, if money were available for grants. The Soil and Health Foundation could be the sponsoring agency. If anyone reading this article would like to contribute to such a study, please write to me in care of the Soil and Health Foundation, Emmaus, Pa. The foundation is non-profit, and contributions are tax-deductible. No salaries are paid to the administrators. The foundation has other projects under study now for which its present funds are committed, but if we could drum up some enthusiasm for earthworms we could get this very important census going also. I estimate that $5,000 would be needed for a minimum effort. There are 270,000 subscribers to ORGANIC GARDENING AND FARMING, so if each one of you sends in 2¢ we will have more than enough to get going. But since it is unlikely there will be such a complete response, we will have to count on a few larger contributions also.

When I started writing this article I didn't intend to end up with an appeal for money. But as I wrote I convinced myself that it is essential that we learn more about earthworms, and the quickest way to get the information needed is to finance an earthworm survey by qualified researchers.

TITLE: Keys, Kings, and Kompanies

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Few people will venture from their homes without first checking to see if they have their keys with them. Locks and keys are such integral parts of our lives that we forget that societies exist where they are unknown. In England the oldest locksmith is the firm of Thomas Gibbons which was founded in 1670. Since that time eight generations of Gibbons' have manufactured locks and keys for countless doors. Prisons and asylums are among their best customers. It would be intriguing to investigate the number of people in the last 280 years who have lost their freedom behind Gibbons' locks. I wonder too if the Brothers Gibbons lie awake at night in fear of open-door mental hospitals and minimum security prisons.

Organizations of all types have heirarchy of keys, some of which give the possessor access to more areas than others. Any employee can open his office door but his boss has the key to all the offices on the floor, while the president has the key to every office in the building. I have found that the distribution of keys is rather similar in all organizations. Most of the low status employees have a small number of "weak" keys; high status figures have a small number of "powerful" keys. The janitor has a large number of "weak" keys so his mobility (the number of doors he can open) exceeds that of the senior employees and equals that of the head man in the organization.

The status or standing of a person in his company or laboratory can be guaged by the formula: $S = \frac{D}{K}$. S is the status of a person in his organization (a high number denotes high status), D is the number of doors he must open to perform his job, and K is the number of keys he carries. Examples: The janitor needs to open 20 doors and he has twenty keys, which means he has a score of one; a secretary has to open two doors and she has one key, which gives her a status score of two; a staff scientist has to open six doors or cupboards, and he has two keys, which gives him a score of three; while the director of the laboratory has to open fifteen doors and he has three keys, which gives him a status score of five. However the president of the company never has to carry any keys, since there is always someone around to open doors for him, so with a K of zero and a high D, his status rank in the company reaches infinity.

Initially only the top man in an organization has a master key and then gradually his deputies and department heads acquire them. Once the number of master keys increases, the president adds a special doorlock that cannot be opened by the ordinary master, and soon the other department heads follow his lead. When the point is reached where the master key opens less than half the doors in the building, the president begins to reassess the situation. He finds that his firm is filled with small empires each with its own Caesar, and it is almost impossible to tell an employee's status by the keys he carries. The president may then try to obtain a grand master which will open all the special locks and, if he succeeds, the department heads who previously had master keys (which have now become sub-masters) will also try to obtain grand masters. The second alternative is for the president to recall the master keys and try to restore the original situation. This means that the special lock on department offices must also be removed.

I worked in one large company where the second alternative was attempted, and the results were illuminating. The department heads ~~who were scheduled~~ to lose their master keys were quite vocal in denouncing the new policy. They were especially concerned that some few others, no more worthy, were destined to retain their master keys. Since I was to lose my master key, and I found myself quite upset about it, I tried to analyze my feelings. Actually I had little reason to own a master key, since the only places I went were my lab and the general company office and the keys I owned were quite sufficient for this. Why did I object to losing access to places where I never went? It seemed irrational but such feelings often mask some basic laws of human nature. For one thing the new policy showed the truth of the axiom that absence is easier to bear than loss. If I had never been given a master key, I should not have minded not having it as much as I did losing it. However I think the main reason was that the policy curtailed my living space. It posted little signs around the building "You can't enter here." Imagine that you receive a note in the morning's mail, "Sir, you can go anywhere in the world except Utica and Syracuse." How would you feel under these circumstances, assuming that you had no desire to visit Utica or Syracuse? I think you'd be quite angry and ask yourself why you were forbidden to visit these places. You'd probably forget that you didn't want to go there and begin wondering "Why don't <u>they</u> trust me, and what is there in Syracuse and Utica that I shouldn't see? What right have they to tell me not to go to Syracuse and Utica? Do they think I am going to steal something or kill the mayor?" You would also feel that you had lost something and actually you'd be correct. Basically your <u>right</u> to visit Utica and Syracuse had turned into a <u>privilege</u> granted at someone else's discretion.

This was the way I felt when asked to surrender my master key. I felt my freedom of action was being curtailed and that the company's trust in me had diminished. I knew that if I had never been given a master key, I'd probably not have minded in the least, but now that they were ~~taking~~ away something that I'd grown accustomed to, I was quite angry, especially when I found that people "lower" in the company heirarchy still retained their keys.

The staff scientists and department heads reacted to the loss in different ways. The dominant reaction was a combination of shame and bitterness. The latter was most evident in discussions with family and friends, while the former emerged in relations with colleagues. One department head who considered the loss of his master as a loss of face, said he attempted to compensate by requesting another, completely irrelevant key. When he was told to turn in his master, he immediately asked for a key to the company auditorium. He confessed that he probably would never use the auditorium, but he craved another key to replace the one he had lost, even if it were able to open a door that meant nothing to him. This seemed to be in line with the belief with psychoanalysts that keys are substitute sex symbols, and that gaolers and attendants with prominent heavy key rings in their belts are simply compensating for undersized sex organs. Another man mentioned his feelings of shame when he was compelled to borrow a key from the office janitor so that he could enter certain parts of the building on Saturdays. For the first few weeks he was unwilling to admit to the janitor that he no longer had a key, but would simply say that he had "forgotten his keys" that day. As for me, I expressed my annoyance with the new policy by writing this article. Of course I also lost no opportunity to annoy the lab director by asking him to open doors for me. Since, in theory at least, the ethics of scientists are egalitarian, such a blatant demonstration of inequality is quite embarrassing to him. It is also a direct reflection on his administrative acumen, since an astute department head should look after

his staff. The fact that "one of his people" has only low power keys indicates that he has failed to raise the general status level of his department.

At coffee a friend showed an enormous string of keys and complained that it tore holes in his pocket. Someone else at the table turned to him and asked to examine this fantastic collection. The enquirer found that there were two copies of a few keys and others that would probably be used less than once a month. My friend defended the duplications by saying that it was easier to find keys when there were two of each and he felt that he needed the lesser used keys because "What if I'm in the hall and someone asks to get into one of the cupboards?" The fact that this wouldn't happen more than once a month didn't seem to matter. He felt he would lose face if someone pointed to a lock that was in his territory and he wasn't able to open it immediately.

Needless to say, I and many of my colleagues adopted the same realistic solution when we heard rumours of the new policy. We immediately went to the nearest hardware store and had duplicates made of our master keys! We then felt secure against the new directive. Events later proved us correct when the company locksmith came to my lab and asked if he might see my keys. When I gave him my key ring, he said he would like to check it and "perhaps" change one or two keys. I acted quite surprised later in the day when he told me that he had replaced my master key with one that would fit only my office. From what I gathered, he also encountered the same look of surprise from the other executives, most of whom are fairly good actors or they would not have risen as far as they had. From that time on, I carried my master key in my wallet and felt just as important as the company president.

TITLE: Some Lines of Basic Research Suitable for a Mental Hospital Setting*

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I must explain, before saying anything else, what I think basic research is as opposed to any other kind of research. Sometimes basic research is described as activity upon which no set of practical goals are imposed, where the investigator is free to go where his curiosity leads him without any thought as to whether his research will have immediate practical consequences. I think this is an exaggerated picture.

I would much rather describe basic research as strategic research and the more practical or applied kind tactical research. Basic or strategic research is characterized as having longer range goals and the expectation of broader applicability than practical or applied research with immediate goals and limited applicability. Thus I think that the two kinds of research do not differ in kind, but only in degree. It is therefore reasonable for me to select from the broad fields of basic psychological research some areas which I think might eventually produce principles with broad applicability to the problems met within the mental hospital setting.

In a sense, we can narrow the fields of basic research by specifying the nature of the problems faced by the therapist. Mental hospitals were invented to provide custodial care. They often attempt to provide therapy. The professional staff of the hospital is almost exclusively concerned with the function of providing therapy. What then is the therapist's problem--stated in such a way that long-range, strategic research can be carried out on it?

Here we become arbitrary. I am going to state the problem one way with full realization that in large part it is the way in which the problem is stated that determines the nature and the course of the research.

If I attend to the behavior of any one of my colleagues for a period of time, especially here in a strange setting, I observe a pattern something like this. He arrives with a purpose. He knows nothing of the hospital layout and little of the people he is to meet. He inquires, he follows, he performs a little trial and error. He learns, he performs, he satisfies his needs, he accomplishes his goals, and he returns home at the end of this meeting. He and each of us are persistently successful problem solvers. Our behavior is a constant process of adjustment and adaptation to the situations we meet in the light of our goals. Our behavior is for the most part socially acceptable.

Now let's look at a few patients. In what ways are they different? Some or all of their behavior is not socially acceptable. If none is, they are on

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*Paper presented at a symposium at the V.A. Hospital in Marion, Indiana, some seven years ago. Prof. Walker asks the reader to understand that much additional research has been performed since which might well be pertinent. For additional comments on this paper, see "Worms and Things" at the beginning of this issue. --JMc.

the back ward. But more important, they can be characterized as having lost to a greater or lesser degree, the capacity to adjust, adapt, and solve ordinary problems. Instead they are more or less inflexible, rigid, non-adjustive, non-docile--at least the behavior to which we object can be so characterized.

If the therapist is intent on symptom treatment, and often this is a legitimate goal for a therapist, he attempts to remove or dispose of the asocial, non-docile behavior and replace it with socially acceptable adjustive behavior. If the patient's difficulties are more extensive, the therapist is more likely to treat the whole organism, to attempt to take an asocial, non-docile person and to recreate an adjusted person who is more responsive to social demands.

Basic research on docility

If the therapist's problem is stated in this way, then docility becomes the focus of basic research. In general basic research on docility can take two forms. One approach is to start with a non-docile organism. The first task is often one of trying to eliminate or erase the offending behavior. Electro-shock therapy is an eraser mechanism which wipes out to a degree and for a time non-docile behavior. The therapist can then substitute more docile and socially acceptable behavior. Eraser mechanism research can be carried out on human subjects and the best of it has been.

Another approach is to try to discover how non-docile behavior can be produced, and then to try to eliminate it. This usually involves starting with a normal, well-adjusted organism, and for this reason, most of it has been carried out on animals.

I would like to tell you about several lines of research of these two varieties that I either know about or have participated in which seem to me to be suitable for the mental hospital setting.

Eraser Mechanisms

One approach to the elimination of inflexible and undesirable reaction patterns is to attempt, by some means, simply to wipe it out and start over so to speak. Insulin and metrazol shock, electro-shock and even lobotomy are based at least in part on this notion. Each seems to produce in part an ontogenetic regression, and if the effects of previous experience can be erased back to the beginning of the disorder, then therapy can steer a new developmental course. Unfortunately, these procedures are all violent, and they are all probably attended by residual damage and impairment.

Very recent sensory deprivation experiments may offer a new approach to the eraser notion, and if they do, as a technique, this approach will have several concrete advantages. Chiefly, it does not produce damage, and it maintains the subject in a state of accessibility to the therapist. No one, as far as I know has attempted to use the technique in therapy, and may not have even thought of it.

Hebb's original research in this area was a classified project done under contract with the Canadian government. As a consequence, reports were fragmentary, incomplete, and possibly even misleading in detail. There was one "leaked" report in MacLain's, a popular Canadian magazine. There were two articles in the Canadian Journal of Psychology, one by Bexton, Heron and Scott and one by Hebb, Heath and Stuart.

In one of these experiments subjects were made to undergo auditory deprivation by having cotton and petroleum jelly ear plugs inserted in their ears for a period of about four days. Hearing was reduced but not eliminated. They could hear lectures but not classroom discussions and apparently made use of lipreading to some extent.

Under this mild form of deprivation several marked effects were observed. Internal noises such as neck creaking and hair combing were markedly loud. The sound of the subject's own voice was so loud that all six subjects suffered acute social discomfort from a consistent vocal output so low in intensity that they could not be heard readily.

Motivation was affected. One subject who planned to study through his four days found that he was utterly unable to do so. Another complained that the extreme hush of the library was unendurable for two days, although this one seemed to recover and was able to study some the last two.

Emotional disturbances were common. The general picture was one of increased irritability, depression, and withdrawal in very mild form, and a marked lack of self-awareness of these disturbances. There was a real discrepancy between the subject's reports in this matter and the reports of friends.

In the more well-known experiment subjects were placed in a lighted cubicle for about three days. There were 22 men subjects. They wore translucent goggles which virtually eliminated pattern or form vision. Their heads rested in a U-shaped foam rubber cushion and they wore earphones. In combination with the partially sound-proofed character of the cubicle, auditory stimulation was reduced. The subjects wore gloves and cardboard cuffs which reached from just below the elbow to a point beyond the finger tips. The subjects came out of the cubicle for food and toilet but kept the goggles in place even then.

Initially the subjects did quite a bit of sleeping and felt almost elated. A variety of mathematical tests and word tests showed decrement at 12, 24, and 48 hours in the cubicle as compared to pre-cubicle tests on the same subjects and as compared to control subjects.

The initial elation was followed by irritation and some subjects gave up in spite of high and much needed pay. During tests subjects seemed abnormally and childishly pleased with minor successes and abnormally irritated and sulky at failures. The subjects seemed to lose the capacity for integrated intellectual activity, could not initiate problems, gradually lapsed into involuntary daydreaming and even reported blank periods when they thought about nothing at all.

Most striking were the hallucinations which developed. In vision the first experience was one of the dark field growing light. Simple dots of light, lines and geometric figures followed. A majority of subjects progressed to wallpaper patterns. A number reported isolated figures without background. One was a row of little yellow men with black hats and with their mouths open. This level was reached by about half of the subjects. A few subjects saw integrated scenes, "a procession of squirrels with sacks over their shoulders marching 'purposefully' across a snowfield and out of the field of 'vision'." Some visual hallucinations were reported as inverted or tilted.

There was partial control of content, but what the subject was trying to see came slowly and was often preceeded by associated objects. The hallucinatior

survived physical activity including conversation with the experimenter, but they had a tendency to disappear when the subject was trying to carry out a complex mental task.

Some subjects reported hearing their hallucinated figures talking. There were tactile hallucinations of pellets hitting an arm and an electric shock from a visualized doorknow. Probably of acute interest were reports by a few subjects of distortion of the body image. One described his mind as a ball of cotton-wool floating above his body. Two subjects reported perceptions of two bodies lying side by side in the cubicle, in one case with a partial overlap.

When the subjects came out of the cubicle and sensory variation was restored, they reported being dazed, visual perception was disturbed for one to two minutes. They had difficulty focussing, objects appeared fuzzy and did not stand out from their backgrounds. There was a tendency for the environment to appear two-dimensional and colors seemed more saturated than usual. They also reported feelings of confusion, nausea and headaches, fatigue up to 24 hours after release.

There are rumors of persistence of disturbances in depth perception lasting as much as a week. Also, there are reports of an abnormal effectiveness of indoctrination carried out while the subject was in the cubicle. They were somewhat unexpectedly induced to believe rather weird statements about the reality of supernatural events. These statements came from recorded speeches which the subject was allowed to play over and over if he wanted to relieve the boredom.

In summary, it seems apparent that some very fundamental experience-produced reactions had been partially erased or disturbed, and control of the character of new ones seemed easy.

I recently visited Dr. Lilly, a neurophysiologist at the Institute of Mental Health in Bethesda, Maryland. Stimulated by Hebb's research, Dr. Lilly has set out to create a much more severe state of sensory deprivation. He had a large tank of water isolated from all noise and vibration, and in which the temperature and chemical composition of the water could be rigidly controlled. He built a latex face mask which was attached to an air supply and which came pretty close to eliminating any pressure gradient on the skin. With the face mask in place, he suspended himself in the tank. Under these conditions changes in the afferent input are pretty near an absolute minimum.

Dr. Lilly had been practically the only subject in this experiment at the time I was there; in fact, he was still in the process of working out the technical aspects of the situation. Here are some of the things he was willing to report. In general, the sequence reported by Hebb occurred in this situation much more rapidly. Hebb had reported that at the outset his subjects had done quite a bit of sleeping. This phase did not appear in Dr. Lilly's experience. Within a short time, a matter of 20 to 30 minutes, the blackness before him ceased to be flat and became clearly three dimensional. In something like 45 minutes visual hallucinations began. They were mainly geometric and were described as luminous and in some respects resembling pictures taken in the depths of the ocean. From this point Dr. Lilly was unwilling to describe his experiences to a stranger, and it is obvious that they had become profoundly personal. There were one or two asides which are of real interest, however. In his early attempts, he used standard masks in his breathing apparatus. Under

other circumstances, such masks can be worn many hours without major discomfort. When removed, they often leave a red mark on the skin at the seal line. In Dr. Lilly's tank, this pressure gradient at the seal line became very quickly excruciatingly painful. On one occasion, the water supply hose began to emit very tiny bubbles at a slow frequency. The hose was just under Dr. Lilly's foot so that the bubble in rising struck the foot, ran over it, and then came to the surface. The sensation was described as being indescribably pleasant--perhaps the way orgasms at a rate of one every 10 seconds might feel. Unfortunately, the frequency of the bubbles increased and the sensation changed to neutral and finally to an intolerable pain.

I might point out that Dr. Lilly is analytically trained and is perfectly aware of the obvious interpretation of the state in which he has placed himself. No experimenter or any second person is present when Dr. Lilly is in the tank. His assistant, his only other subject, also enters the tank alone. Dr. Lilly's aim is to eliminate the obvious dependency relationship which would be unavoidable with a second person present.

In many respects, this mechanical sort of sensory deprivation, as an eraser mechanism, seems more exciting and frought with possibilities than any other technique. The changes are profound, fundamental, and in Dr. Lilly's situation, appear in a short time. A second person, a therapist, in the situation apparently could wield more influence than previously has been thought possible. Unlike other eraser techniques, this one involves no damage to the organism, and the patient is constantly accessible--possibly more accessible than a patient has ever been before.

How could research in this area be done in a mental hospital setting? This area is so new and so obviously full of possibilities, that I don't see how one could miss, provided he had the spark of interest and ingenuity without which no research is possible.

Produced non-docility -- A temporal paradigm for masochism

We are all familiar with diagrams developed by Lewin and subsequently by others which are designed to demonstrate the dynamics of situationally induced locomotion determined by the topological location of objects of positive and negative valence. Positively valenced objects elicit approach behavior, negatively valenced objects avoidance behavior, and a variety of predictions can be made by arranging patterns of objects of various strengths of valence of each sign. Let us apply this kind of diagram to something like masochism.

The most peculiar characteristics of a masochistic act is that the organism involved endures pain that is seemingly needless. Interpretation often takes the form of specifying that in some manner the masochist derives pleasure in some devious way. This can occur in two ways if we think of the person being involved in an inevitable sequence. He has a presumed choice of starting or not, but once started, must complete the sequence.

One way would be that the sequence starts with an initial pain followed by a much greater state of satisfaction. He feels guilty, he punishes himself and the pleasure and relief of expiation is worth the pain involved. Essentially we are saying that this behavior is adaptive, and we try to eliminate the guilt to eliminate the satisfaction, and the self-inflicted pain will come to occur.

The other arrangement involves an immediate pleasure followed inevitably by a painful experience. The degree of pleasure must exceed the degree of

anticipation of pain, or the behavior would not occur. The therapist usually tries to decrease the pleasure, increase the anticipation of pain, or to help the organism find a source of pleasure that does not involve so much inevitable subsequent pain.

These are standard and as such things go, would be relatively easy to treat. Occasionally, however, another pattern is met. The organism behaves masochistically, yet no sequence can be discerned. The organism seems compelled to perform over and over again an act that obviously involves great pain, not unmixed with a little pleasure. From the objective observer's standpoint it looks as if the sum is exceedingly painful.

A few years ago I ran into an article by Frank Beach in which he reviewed great numbers of studies of sexual behavior. One was intriguing as a possible beginning point on the third type of senseless, non-adaptive masochism described above. Male rats were rigged so that they could be painfully shocked at any time even though they were free to move about. They were placed in a cage with a receptive female. Some of the males were shocked whenever they started to approach and mount the female. Those animals promptly learned to avoid females altogether. Thus the pain standing inevitably before the pleasure was sufficiently strong to counter-act the positive valence involved. With a lesser shock, the animal would not have avoided the female, and even in this case, without certain knowledge of the pleasure involved, we would say that this wilful submission to shock was masochistic. In other words, the pain was boosted to a value which eliminated the first type of masochism I mentioned above, and the animal's behavior seems adjustive. Other male rats were permitted to mount and copulate. These animals were shocked as they dismounted. These animals, too, learned to avoid females altogether. In other words, the ultimate pain was great enough that the anticipation of pain in the presence of possible immediate pleasure was sufficient to prevent the sequence. This is a development of the second pattern above, and the behavior is again adaptive.

The third pattern gives us pause--something new had been added. These animals were permitted to mount the female and begin copulation--and then the shock was administered. The effect produced was as follows. The male simply proceeded to copulate more vigorously, almost violently, and never showed evidence of female-avoidance with repeated experience. He may have, in fact, acquired a senseless appearing, non-adaptive masochistic act.

The problem, and a clearly basic research one at that, is why? The same shock, either fore or aft produced avoidance. Is there something about the simultaneous administration of pleasure and pain which produces something other than an algebraic sum? Does shock administered in this way simply increase activity and thus increase pleasure? Is something happening here that if explained, could also explain the most irascible of the masochistic behaviors?

We have studied one small aspect of this problem in the laboratory at Michigan. We worked with a linear maze in which shock could be interposed between an animal and food. We discovered that with a shock sufficient to bar the animal from food, he would take the shock to return to the starting place. We have completed one study of this phenomenon. We have not as yet attacked the main problem. It could be attacked with the facilities at hand in a mental hospital. To say <u>how</u> would require ingenuity and more time than I have here.

Avoidance Conditioning

Avoidance conditioning may be a second paradigm for masochism as broadly defined, and again may turn out to be the same one with which we have just been dealing.

The problem as I see it is this. In classical conditioning, once the response is established, omission of the unconditioned stimulus leads to extinction. In avoidance conditioning the procedure differs from the classical in one important respect. In the former, during training, the UCS is always administered to the animal, whether he makes the response or not. In avoidance conditioning, the animal is permitted to start his own extinction session by making the response. This procedure often leads to high resistance to extinction. High resistance to extinction in the absence of reinforcement looks like a compulsive persistence of some human ritual-like acts long after they have ceased to have any adjustive function. They serve no objectively present need and achieve no relevant goal. Why do they persist in this stupid fashion?

No one now knows, but principles of therapy may be considerably advanced when someone finds out. Let me give you one tentative working hypothesis which may prove fruitful.

Suppose that anything which occurs contiguously with a very intense pain is learned to a very high degree even in a single trial. The shock that produces the intense pain in the dog also makes him lift his leg reflexively at the same moment. For this reason, leg lifting is learned to a high degree and is fixated in a sense. It will persist for a very long time under continued training without reinforcement or without further training. I once finished with a big bird dog in the laboratory. This was nearly twenty years ago at Indiana. He was too good a bird dog to destroy. For the fun of it, I set up in the dog a foot lifting avoidance response. A few days later, I took him over in the hills of Brown County and gave him to a farmer I called down out of a high field. Two years later I went back and borrowed the dog. In the laboratory, the avoidance response occurred promptly and without apparent diminution on the first trial and through a session of twenty trials. Of course, there is no self-inflicted pain in this, only stupid persistence.

Solomon conditioned dogs to jump from one compartment of a box to the other when the light went off in the side the dog was in. The UCS was a just sub-tetanizing shock. Once the response was set up, "reality testing" contrived by placing a glass plate over the access hole did nothing to eliminate the response. There had been no shock for a long time. When the glass was removed, the dog jumped without waiting to see if the shock was going to come on. Solomon also tried punishing the response. This he did by electrifying the floor in the other side from the animal. If he jumped, he received a good stiff shock when he hit the other side. All this did was to shorten the latency of the jump. The animal would apparently rather take the shock on the other, or light side, than wait to find out whether there was going to be a shock on the dark side.

This seems like stupid, rigid, fixated and now masochistic behavior. If you accept the principle that intense pain plays this role of fixation, then why doesn't the classical conditioned response persist? Why does one procedure produce seemingly adjustive behavior and the other not?

At the start of training in both procedures the shock is administered when the animal has his foot in the resting position. Foot lifting is elicited. In avoidance training this condition never varies. The shock is always administered when the foot is in the resting position or when on the rise and the natural response to shock is to lift or to continue lifting. In classical conditioning, as soon as the response starts anticipating the shock, shock begins being paired with further lifting, with the foot in the withdrawn position, and with the foot coming down. The response to shock in these conditions is quite different. Sometimes it is extension, sometimes holding still, sometimes other responses not involving the foot at all. No one response is uniquely paired with pain, no one response persists.

At one level at least, this would explain the difference between the effect of the two procedures. It also does something else. It suggests a cure for the persistent avoidant response and thus perhaps a principle of therapy. If by hook or by crook one manages to pair the pain with another and incompatible response, the integrity of the pain-foot lifting situation can be broken. How are you going to do it? Ingenuity must be applied by each therapist to each situation. In a few months I am going to try to cure a few dogs of foot-lifting. I'm going to deliberately rig the apparatus so that shock will now be paired with other aspects of the response. It might be delivered when the foot is up, or when it is coming down and thus might fix an incompatible response.* Another technique would be one suggested by Guthrie for another reason. Elicit foot lifting in massed trials until the response fails. Then elicit an incompatible response (lifting both hind feet) in the presence of the original CS. I am going to come back to a simpler variation of this procedure a little later.

These two taken together, at least as I have interpreted them, would justify a statement something like this. Living organisms possess a beautiful adjustive mechanism. Most stimuli which induce intense pain also reflexively produce responses which get the organism away from the source of the pain. This response is learned so well that the organism, if he survives the first encounter, never meets that particular pain again. This mechanism always produces rigid, inflexible behavior, and in an extremely large proportion of the instances in which it operates, the effect is adjustive. In a few cases where an adventitious set of circumstances produce a unique pairing of a response with a cue that actually does not happen to bear an invariable relationship to the pain, the effect will appear asocial and maladjustive. It can be eliminated only with a great difficulty, and the above are only untested guesses as to how to do it.

Produced Non-Docile Behavior Involving Choice

Another way to approach the problem of rigid inflexible behavior is to examine situations involving choice between alternatives rather than a choice of doing something or nothing. This time, we shall omit the characteristic that the behavior in question must of necessity involve self-inflicted pain. It could, but most of the data don't fit this criterion as an end result.

Russell DeValois did a dissertation at Michigan a while back that related drive strength to variability. He had a maze built out of five diamonds connected by short alleys. The rat had five successive choices of left or right,

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*The effort proved successful.

but the distance to the end was the same no matter which way the rat turned at any point. DeValois had a variety of measures of variability and all showed that the stronger the drive the less the variability. He had a group on six hours of thirst with full water reward at the end. One group was on 22 hours thirst. Another experienced light shock everywhere in the maze except in the goal box at the end, and another group received strong shock. Each animal had one trial a day, and I think that is important. The animals running under strong shock exhibited very little day to day variability, and were much more likely to choose a pattern of llllll or rrrrr than other animals.

This looks like strong support for the notion that any response paired with intense pain is pretty much fixated. You would almost guess that these animals would have run through the stereotyped pattern under shock even if a painless alternative was open to them. This has not been tested in this setting.

We have just finished another study which may be relevant. I have been playing around with a neural mechanism to account for the efficacy of reward and punishment. There is no time to go into details about it here, but it seems to fit a lot of psychological data, and is not inconsistent with current neurophysiological data and hypotheses. Previous such mechanisms such as Troland's beneception called for a direct lowering of synaptic resistance. Ordinary neural activity produces a temporary raising of the threshold and a return to normal with time. The mechanism I have been playing with simply facilitates or enhances whatever is going on anyway, and both punishment and reward should produce this facilitation. If this is so, the immediate effect of reward would be to raise thresholds rather than to lower them, and after a period of time they should recover and finally show a lowered value. To translate this into a real or palpable situation, suppose that we put two thirsty rats in a T-maze. Both turn left. One rat receives nothing in the goal box, and he is returned to the starting point and given another trial. The raised threshold in the system corresponding to the first reaction of turning left, should make the animal now turn right unless he started with a strong preference for the left side. About 80 percent of the animals do turn right or alternate, and the longer you wait between the two trials, the more time the action system has to recover, and the less likely that the animal will alternate. Now suppose that the other thirsty rat receives water when he turns left. If the mechanism works as we said it should, the reward should facilitate the action system involved, and this animal should have a greater tendency to turn right on a subsequent trial, or even <u>less</u> tendency to return to the rewarded goal box than the unrewarded animal. This turns out to be true too. Roughly 90 percent of rewarded animals alternate. This advantage over the unrewarded rat lasts some time as tested with various time intervals between trials. If one waits long enough, the rewarded rat must eventually recover from this action decrement and then show that he acquired a small increment in habit strength as a function of reward.

One would guess that the stronger the drive-reward pair (thirst and water or shock-escape), the greater the facilitation, the greater the action decrement, and the greater the eventual increment in the response strength. DeValois had a day between trials. If he had had only a few minutes, his strong shock animals might have shown more variability than any other group.

What does this have to do with therapy--even as basic research? Well, these data and hypotheses demonstrate a weak point in a rigid response pattern.

The stronger, the more inflexible, the more rigid the response, the greater the action decrement following the response. Immediately after the response has been elicited, it is subject to manipulation, and probably in ways that are available at no other time.

Therefore, we have some general principles of how such rigid inflexible responses can be set up, how they are probably set up under real life social conditions, and have turned up a chink in the armor.

Action decrement develops and dissipates in time. While I have no data at this moment which will establish the usefulness of action decrement in eliminating inflexible behavior, I do have some data which illustrate a complete reversal of effects of a procedure with a simple modification in the timing of events. In one case pain is paired with a reaction, and in another case it is paired with a similar reaction, but only after it has gone into a decrement occasioned by its own occurrence.

Perceptual Defense Studies

Some of you, by this time, have probably developed some variation of the reaction, "Aw, that may work on animals but not on human beings." I think it will certainly work as well or even better on human beings than with animals although I can't exactly prove it.

We have, however, carried out this pair of studies on perceptual defense which at least fit into the pattern. The principle is, I remind you, that any intense pain and relief paired precisely with a reaction will enhance or facilitate the reaction. If the reaction is a perceptual one, enhancement should have the effect of making that percept somewhat more vivid than a similar one not so paired. Tom Pustell tested this recently. He used a Gerbrand's tachistoscope. In it he had a card with a fixation point at the center and four figures equi-distant from the center with one up, one down, one left and one right.

He first asked subjects to find the square, or the circle or the diamond or the triangle, and thus got a rough measure of level of awareness. He then shortened the exposure time to produce a very low level. Then he ran a series in which the subject was to report which figure stood out the most (responding with the position since at those speeds they hardly know what they are seeing). This established a baseline of preference. Then he switched cards. This new set had one figure on each card a little darker than the others, and it thus objectively stood out the most. The subjects selected the darker figure more than 95 percent of the time. Pustell then selected one critical figure for each subject. When that figure (the circle for example) was actually darkest and thus stood out the most, the subject was shocked as this card was exposed; actually, .3 seconds after the beginning of the exposure. When others were the darkest, there was no shock. He used partial reinforcement with the critical figure. After this training procedure, the original cards were again placed in the apparatus with all figures having the same stimulus value. If the hypothesis is right, the subjects should now have chosen the critical figure more often than the others. His men subjects did quite clearly, both in terms of chance selection of 25 percent and their own frequency of selection of that figure before the training. I think his women subjects showed nothing. After cutting the data two or three ways on the basis of level of awareness and other variables, Tom became convinced that they showed some perceptual

suppression or defense. I am satisfied that the hypothesis holds in this case even though the shock is really not strong and the training period is short.

The second study was that of Don Dulany. He followed along with Tom, but changed the procedure. He never used the cards with the emphasized figure. He geared the appearance of shock to the subject's response. In one condition (Defense Training) a critical figure was chosen by Dulany, and if the subject on a given trial selected the position in which that figure appeared (up for instance), he was shocked. On that same trial, if he had selected the position of any other figure, he would not have been shocked. The critical figure moved around from trial to trial, of course. Under these conditions, the subject came to select that figure less and less often. In a test series with the shock apparatus removed and the subject instructed that there would be no more shock, it still stood out the least. It had undergone perceptual suppression even though apparently paired with shock. In another procedure (Vigilance Training), the selection of the position of the critical figure was followed by no shock, while the selection of any of the others led to shock. These subjects came to choose the critical, unshocked figure over the other three. Again the figures apparently paired with shock faded, while the single figure not so paired came to stand out. This was perceptual selection.

Now the principle worked in Pustell's study, but is in a sense quite sharply contradicted in Dulany's. In both procedures of the latter study, it is the figure or figures which were not paired with shock which came to stand out. Now what is the critical difference between the two studies?

In Pustell's study the "stand-out" character of the critical S occurs in very close coincidence with shock. Therefore, it was this percept (the critical S standing out and the others subordinate) that was facilitated and thus fixated. In the Dulany studies the shock was delivered two seconds after the beginning of the exposure. A fraction of a second is consumed in the development of the percept. Presumably the subject honestly reports which figure stands out the most in this initial percept. It is well known that the span of attention is about .5 seconds on the average, and there is no way I can see to separate the "stand-out" character of one of these figures from selective attention to it. It would be a normal expectancy that attention to one would be followed by attention to another, and that for the most part, the subject might attend to each of the four in turn before returning to the first. One need not go that far to determine that the figure the subject reports as standing out will never be the one that is standing out when the shock is delivered. Each of the others has a probability somewhere between .33 and .5 of standing out at the time of shock, the actual value depending upon whether more than one figure can "stand-out" at one time. This analysis predicts Dulany's results precisely, from the original principle that any percept or reaction precisely paired with shock or pain would be facilitated, strengthened, and tend to become relatively dominant. Dulany paired shock with the action decrement produced converse of the figure the subjects report as "standing out." This analysis was made:

1. To establish the feasibility of the original principle.

2. Since shifting of attention, of the involuntary sort, at least, is a function of action decrement, this pair of studies serve as an example of a complete reversal of effect which can be produced by making use of the chink in the armor of inflexible reactions occasioned by the deep action decrement.

3. It demonstrates that the principle can be applied to perception and not just to overt responses or reactions.

4. It demonstrates that relevant basic research involving the production of inflexible behavior can be carried out on human subjects if you don't go too far in establishing rigid, stereotyped reaction patterns.

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I have been trying to make clear that one way to make basic research relevant to mental hospital programs and problems is to state the problems broadly. If you say you want to learn to erase or wipe out undesirable reaction patterns, or that you want to learn to set them up and then wipe them out, who can deny the relevance of the problem?

If you state the problem in such a broad way, there are literally hundreds of ways in which the problems can be attacked. It only requires ingenuity to attack them with the facilities at hand.

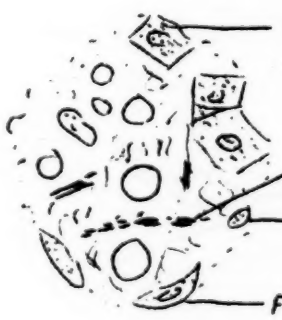
TITLE: A Little Structure Goes a Long Way

ARTIST: Reeva Jacobson
Planaria Research Group
University of Michigan

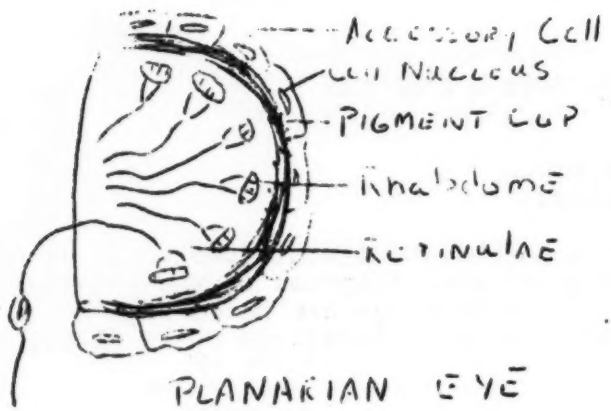
For those of you who have wondered about what goes on under those two corssed-eyes, we present some diagrams of the internal structure of the planarian. These drawings are based on pictures in Taliaferro (1920), Jennings (1904) and Hyman (1951).

EXTENT OF PLANARIAN DIGESTIVE SYSTEM

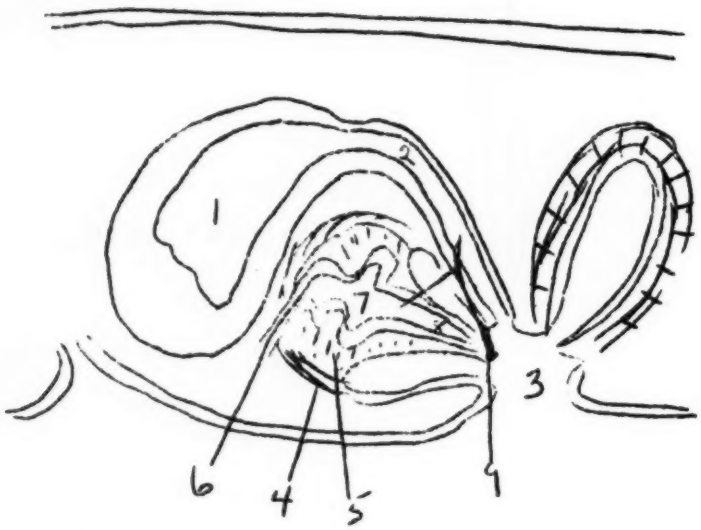
PLANARIAN NERVOUS SYSTEM



MESENCHYME (PARENCHYMA) of A PLANARIA



PLANARIAN EYE



COPULATORY APPARATUS

1. copulatory bursa
2. bursal canal
3. common gonopore
4. penis bulb
5. glands of penis
6. sperm ducts
7. bulbar lumen
8. EJACULATORY DUCT
9. COMMON VITELLINE DUCT

COPULATION

THE WORM'S-EYE POINT OF VIEW

(The title of this feature is taken from Ernie Pyle's HERE IS YOUR WAR: "I write from the worm's-eye point of view.")

FROM: L. B. Womack, Director
Missouri Earthworm Research
(All You Need Is a Can of Worms!--Reg. Trademark)
Box 6851
Kansas City, Missouri

Have read the article in the New York Sun Times by Earl Ubel. Obviously you can understand the interest I would have. Would you please advise how to obtain the book entitled WORM RUNNER'S DIGEST?

(Herewith enclosed. And this time we even have an article about earthworms!--Ed.)

FROM: Christine Curlee
804 Verona Court
San Diego 8, California

I hope that it will please you to know that I placed first in my School Science Fair, and I have been selected to go to the Greater San Diego Annual Science Fair. This will be held in Balboa Park, April 14-18. The judges were very interested in my project, as I do not believe that anyone is doing work with worms and conditioned responses in San Diego, at this time, except for me. They were especially interested in your letters which I included in my work-book.

I received a new shipment of planaria from Turtox on the 25th of February. After a week three of them have developed forked tails. Could you please explain this?

At last testing, some of my planaria which had regenerated naturally from a fourth "generation", conditioned batch, reacted. These were fifth "generation" worms. I am really curious to see if I can keep them alive through a sixth "generation" and see if they will react.

Thank you again for all of your help. I plan to keep on working with my worms, and I will write now and then if something new happens.

FROM: Hettie Cattell
Feature Writer
New York Mirror

Thanx 1,000,000 for the Worm Runners Digest!!!

I ADORE it!

Long having felt that the psychologists--and others--have taken themselves and their experiments too d----d seriously, I get a terrific bang out of the Digest.

I especially welcome the slant on psychology--and "social science" experiments. Even the term "social science" annoys me beyond measure. Hownell can there be a "social science" when psychology (on which it MUST be based) is NOT a science--and God knows if it ever will be one?

Anyhow, cheerio to the boys of the planaria group research.

(The above letter was not sent directly to us, but we confiscated it anyhow. Since half of the planaria research group is female, we'll assume Miss Cattell meant "cheerio" to them too.--Ed.)

FROM: Isadore Rubin, Asst. Publisher
Sexology Magazine
154 West 14th Street
New York 11, New York

We were very much interested by a press report of your tests on the flat worm and wonder whether we may have a copy of your magazine "The Worm Runners Digest."

FROM: Ted Gurr
The American Behavioral Scientist
Post Office Box 294
Princeton, New Jersey

Thanks for the copies of the Digest, received and forthcoming. I think I can sneak an exchange subscription past our business manager--after all, she is my wife. For a psychologist, at least, the title of our journal (The American Behavioral Scientist) may be a bit misleading. We are presently trying to encompass a little bit of everything, from all the social sciences. A more appropriate title, for those who choose to make niggling distinctions, might have been The American Social Scientist, but the initial business simply doesn't work. Hence, ABS.

This journal began 3½ years ago as Political Research: Organization and Design (PROD), "An Informal Newsletter of Research Ideas," in photo-offset. In those early days we often took ourselves less than seriously, as you may gather from some of the earlier issues (coming under separate cover). The editor has consistently avoided institutional affiliations for the magazine, on grounds that it would cramp our style. This has meant subscription prices, however, and a series of changes--culminating in the new title and new format last September--in hopes of making it financially self-sustaining. Like Alice-Through-the-Looking-Glass, we've been running frantically just to keep even. And we also wanted some of our material to be taken more seriously. In the process, however, we seem to have frightened off the lighter material--it isn't submitted any more, alas, perhaps also alack. We do get cited these days, and even reprinted. But I at least sometimes feel that we left a bit of a vacuum behind us--so the existence of WRD is cheering.

In a way it's too bad you haven't left traces at Princeton, etc. The Princeton Psych. Library, for example, is altogether too staid--a condition that

may well frighten off potential recruits. If there is any moral in the above comments, it would be to keep the WRD informal--which doesnit mean it couldn't be circulated to libraries for a token subscription price. (We've had multiple rates from the beginning--one price for individuals, a higher one for libraries--with but a single querilous reaction from a librarian.) As far as a profound title is concerned (incidentally, couldn't you refer to it as THE WORD?), I recall that a new British medical journal dealing--seriously--with the treatment of intestinal diseases has the unadorned, descriptive title Gut. If they want to reference, let 'em reference.

(Last fall, we sent copies of the DIGEST to a select list of behavioral scientists overseas, telling them of our existence and asking them to write us if they wished to be put on our mailing list. We regret we cannot publish all of their replies, but the following are a sample of the many heart-warming letters we received. The "worms" are now truly international.--Ed.)

FROM: Dr. N. Ladygina Kohts
Darwinian Museum
Moscow, USSR

I received to-day, the 22 January 1961, your letter and the NN 2 and 3 of the publication: "A short magazine on worms"; I am deeply grateful to you for your having sent me these highly interesting materials.

I beg you instantly to send me, if possible--for the future too--some reprints of this magazine; we shall be thereby very indebted to you. As a kind of symbolic compensation, I send you one of my latest papers on comparative psychology, which deals with the "Constructive and instrumental activities of the Primates /Chimpanzee/".

With collegial greetings.

FROM: Yasushi Fujita
134 Minamisawa-Gakuenmachi
Kurume-machi, Tokyo, Japan

Thank you for your third and fourth issues of "The Worm Runner's Digest" in reading which I enjoyed myself very much, perhaps much more than you had expected and hoped in your letter. To say nothing of other reasons, why your digest delighted me so much, the spirit of humour based on the love of science and humanity, which is sparkling in and between all the lines, struck me entirely.

From the biological stand of various aspect (Regeneration, Tropismus etc.), I have interested in the flatworm, is the second reason. Particularly, Planaria gonocephala (which is most popular in my vicinity) is one of my most favorite animals, not only as an experimental subject, but also almost as my "pet."

At the first sight of your report, I thought of the words of Draparnauld, cited by Harriet Randolph in the Archiev fur Entwickelungsmechanik der Organismen

Band 5, in the year of 1897. Allow me to repeat here, "Bonnet en parlant de l'ame des animaux, dit que lorsqu'on divise un polype c'est dans la tete que reside l'ame apres la section. L'on pourrait demander aux Psychologistes dans quelle des deux moities de notre Planaire divisee longitudinallement se trouve l'ame apres la section."

The third reason is, your educational concerns and love for youngsters, which is shown in the story of the Penfield Auxiliary. To explain this, I must relate something about myself.

As a student, I learned Zoology in Hokkaido University at Sapporo, then interested in Comparative Psychology, learned psychology in the advanced course of Tokyo University. But it was just pre-war ages of Japan, and I could not mind to continue my life of investigation normally.

With the love for science, natural world itself, and mostly for human-being, I became a teacher. Now, I am teaching the lessons in natural science and mathematics, in a junior high school in my town. The study and application of various psychological tests and the observation of children with the eye of clinical psychologist, amuses me nowadays.

If you please, let me ask you to send me, also, your first and second issues, if possible. And, I would like to appreciate your acquaintance.

With my hearty concerns to your digest.

FROM: J. Lecomte
Station de Recherches Apicoles
Bures-sur-Yvette, Paris, France

Your "Worm Runner's Digest" arrived today, for which I should like to thank you. I was a friendly reader of this informal and unformal paper and, now, I am delighted to know that I shall obtain my own issues.

FROM: Bob Bustard
Gatty Marine Lab.
The University, St. Andrews
Fife, Scotland

I cannot say how fascinated I have been by several copies of "The Worm Runner's Digest" which Dr. Homidge here has lent to me.

I feel that the work you are doing is wonderful and you have certainly whipped up enough interest in it. I wonder if I might perhaps be added to your mailing list? I work on learning and behavior (at least in Insects) and find your ideas and papers of great interest. I would very much appreciate any back issues you may have available.

Please note that as from 1st Sept. 1961 my address will be: Dept. of Zoology, University College, Gober St., London W.C. 1.

Over the next three years I shall be spending an annual 6 months at the American research station in Panama or Trinidad and hope it will be possible to visit colleagues in the U.S. and would like to think I might make Ann Arbor before too long.

I would assure you of the interest which your "Digest" has aroused in the behavior group here. Any reprints of papers if you can spare them would be much appreciated and we'd gladly reciprocate.

With every good wish for the future.

FROM: M. le Prof. Deleurance
Faculte des Sciences de Paris
Laboratoire d'Evolution des Etres Organises
105 Blvd. Raspail
Paris 6, France

I do not object to receive irregularly your Worm Runner's Digest, though I am not quite sure to be able to handle it the right way.

FROM: Toshiaki Sato
Fukushima Medical College
Fukushima, Japan

I received your Worm Runner's Digest, Vol. II, No. 2 and 3, with many thanks. I have read much about Psychology, but none is so interesting as these. I enjoyed them very much. I should be very pleased if my name could be kept on your mailing list. God bless the Worm Runner's Digest!

FROM: J. Medioni
Laboratoire de Psychologie Animale
Rue de l'Universite
Strasbourg, France

Je vous remercie vivement pour l'envoi du Worm Runner's Digest dont la lecture m'a beaucoup interesse. Bien entendu je connaissais deja vos premieres experiences de conditionnement de Dugesia, qui sont du plus haut interet.

Je souhaiterais vivement continuer a recevoir votre publication et meme, si possible, avoir le n° 1.

Je vous adresse par pli separe quelques publications du Laboratoire de Psychologie animale concernant le comportement des Planaires.

Avec mes remerciments, veuillez croire, mon cher Collegue, a l'expression de toute ma sympathie.

FROM: D. W. Ewer
Department of Zoology
Rhodes University
Grahamstown, South Africa

It gives me and the entire staff of my Department the very greatest pleasure to acknowledge receipt of Volume II No. 2 of the Worm Runner's Digest and to assure you that your publication is one of the very few scientific works that is not only acclaimed throughout the department but welcomed as reflecting the only rational approach towards matters of the highest seriousness. With best wishes and many thanks.

FROM: Robert C. Landes
8100 Niles Ave.
Skokie, Illinois

I have read several articles regarding your inheritance of learning experiments with Dugesia. I thought you might be interested in some experiments I carried out as a part of my M.S. thesis work. Dugesia tigrina, regenerating in solutions of Picrotoxin, Benzedrine, Phenobarbital, and Thorazine, showed no abnormalities in the regeneration process. This was in concentrations of the drugs which had previously been shown to stimulate or depress the worms' behavior. The results were taken to indicate that the level of activity of the remaining nervous system is not a major factor in the regeneration of Dugesia tigrina portions.

Whether or not these experiments are related enough to your work to be of any importance I don't know.

FROM: Paul M. Packman
18 North Taylor
St. Louis 8, Missouri

In the November issue of "The Worm Runner" I noticed an appeal for help from Ruth Ziegler of Roanoke, Virginia. She seemed to have some difficulties in convincing her worms to imbibe some gamma amino butyric acid (a CNS depressant) in order that she may investigate the effects of this compound on the conditioning of her subjects.

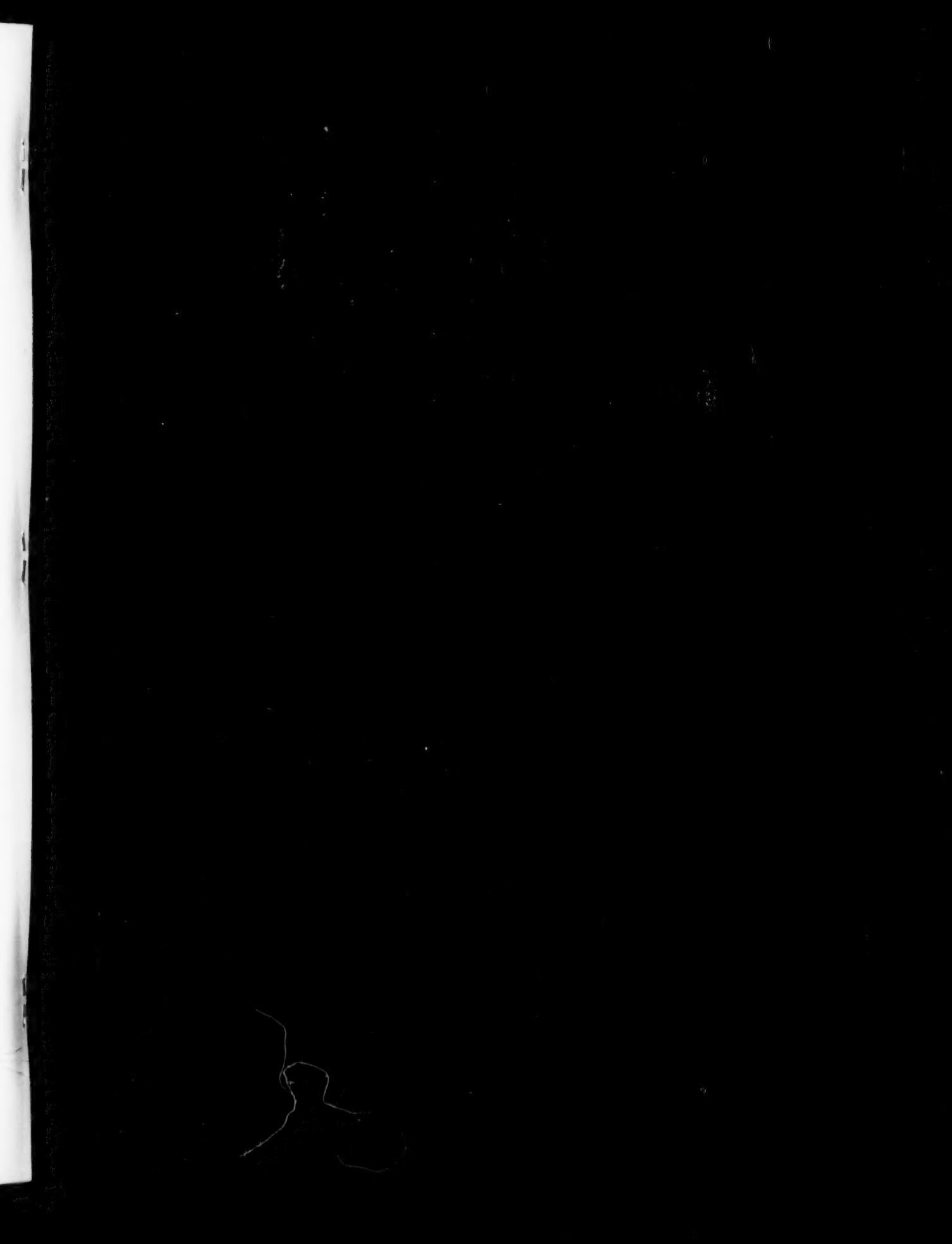
In response to her "frenzied cries" the following possible solution may prove of some aid to her and all others who may be interested in convincing their worms to eat something which the worms may or may not like.

Since most drugs either affect the worms permanently or not at all, it might be possible to convince them (since injections are both difficult as well as almost impossible) to eat the compound. The most obvious difficulty is, of course, how?

Since part of the standard diet of these creatures is beef liver, an interesting idea might be to load the liver with the drug in question. A rather simple possibility might be to perfuse said liver with the drug. However, this, itself, is fraught with difficulties: whoever heard of anyone perfusing a cow's liver? The solution to the problem, then, is quite simple. If the worms will

eat either rat or mouse liver, then there would be no difficulty in dissecting that organ with its major circulatory supply intact. By pumping through the inflowing blood vessels (preferably by gravity), one can fairly saturate the liver with the compound in question and feed it to the worms.

While this on a superficial basis seems simple there are other problems. In order to quantify (the bugaboo of science) the amount of drug there are two possibilities: first, one might take a portion of the perfused liver, homogenize in a waring blender or a glass homogenizer (much as the biochemists do) and assay for the amount of drug present; or secondly, if one is willing to sacrifice several of the worms homogenize them and determine how much drug is present in them.

Of course the above procedure may take a relatively small amount of time. It is up to the investigator to determine whether he or she is willing to invest this in order to produce some decent research.

P.S. Please put me on your mailing list. The articles and comments are quite interesting.